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VIEWS

UPON

THE STATICS OF THE HUMAN CHEST,

ANIMAL HEAT,

AND

DETERMINATIONS OF BLOOD TO THE HEAD.

BY

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TO

BENJAMIN TRAVERS, ESQ., F.R.S.

SURGEON EXTRAORDINARY TO THE QUEEN,

SURGEON IN ORDINARY TO HIS ROYAL HIGHNESS THE PRINCE ALBERT,

AND MEMBER OF MANY LEARNED SOCIETIES.

MY DEAR SIR,

There is no member of our profession to whom I could more appropriately dedicate a work, inviting attention to certain views in physiology, and to the pathological deductions flowing from them, than an author whose valuable researches in the latter science manifest also a profound acquaintance with the former.

Not to know you by your works would argue in any one an imperfect professional education. To many you must be likewise known, as to myself, by a generosity combining a moral with an intellectual superiority. Twenty years have elapsed since my youthful labours were encouraged by the opinion you kindly expressed upon my essay on the mechanism and action of the muscles.*

Since my return to England I have witnessed the same liberality of sentiment accompanying those mental qualities, which have long given you a name, and a position, in the profession, second to none in importance. To these motives is added, that deeper obligation which, in common with many another parent, I owe to your skill and judgment, as the means by which a son has been preserved to me in vigorous health.

I beg you to accept this expression of feeling, not as any adequate return for my obligations, but rather as the avowal of an opinion which all who know you must be prepared to make.

Believe me to remain,

My dear Sir,

Ever sincerely yours,

JULIUS JEFFREYS.

* Note, p. 232.

PREFACE.

The moralist who would study the human mind in its various aspects, or the painter who would command as extensive a knowledge of the human figure, must travel to every clime, or acquaint himself with the works of those who have.

The same course we may affirm to be equally requisite for the physiologist, who would not limit his acquaintance with the functions and powers of the various parts of the human body, to the knowledge he is able to collect from observations made in one clime, and upon one race only.

But who are they that visit distant lands to study this important part of human nature? or what works have we from whence, at our own homes, to derive such information? It must be confessed that our physiology is nearly all drawn from inquiries made in one region of the globe — Europe. There are doubtless some laws in this science of so absolute and universal a character, as to be little affected by outward causes; but these are not numerous. The greater portion of what we consider the laws of our science may, and even must, assume modified forms, under the influence of such powerful outward causes as are comprised under *Climate.*

To none does this remark more forcibly apply, than to the laws governing the production and dissipation of animal heat. The physiologist, travelling to the *tropics*, is therefore placed in circumstances peculiarly favourable for having his attention drawn to many points which escape the observation of his brethren in Europe. To such opportunities I would refer the following "VIEWS," and whatever of value they may possess. They would have been published earlier, had not my time been much occupied in practical details, to which these physiological views, in conjunction with certain *pathological*, gave rise.

Inviting, and apparently more elevating, as pure science may be, we shall do well to measure our occupations rather by their utility to others; and therefore to regard the employment of some portion of time in the practical applications of science, as more really honourable, than an *entire* devotion to theoretical speculations, or an *exclusive* searching after new principles: and in support of this opinion we may appeal to the highest of all,—a Divine example. We read in the book of Nature, as well as in that of Revelation, that the GREAT AUTHOR of all science, compared with whose mind those of the dullest and most enlightened of men are but equal, equally feeble;—that HE, the CREATOR of all things, is an ever-watchful purveyor to his creatures; engaging in the humblest occupations to supply their necessities, and attending even to their most trifling comforts! What a contrast this to weak and helpless man, whose pride is surpassed only by his infirmities and wants!

Time further occupied in giving a practical utility to other principles in Physics, has caused the present publication, which is chiefly theoretical, to be deferred.

I announced it a year ago to form part of a work embracing pathological matter connected with affections of the chest; but I was led to separate the two, and to publish a portion of the latter in a series of Papers on Artificial Climates, which appeared in the "Medical Gazette" last year, reserving the present matter for publication this.

In the mean time the work* of Professor Liebig of Giessen has appeared, treating upon several of the same points, and announcing, upon two or three of them, views similar, in all respects, to certain which have long had a place in my notes. That work, as every thing proceeding from so high an authority must, has attracted in this country great attention. So far as I find my views in accordance with his, I may rather feel satisfaction that they have had his prior confirmation of them.

* Organic Chemistry of Physiology and Pathology, 1842.

The numerous references I should have had to make to the work in question have led me to prefer leaving my own matter as it would have stood, and adding an Appendix, containing an examination of such only of Liebig's views as relate to questions discussed in this volume.

Wherein I find myself to agree with him, much pleasure has been afforded me, and I have duly noticed.* At the same time I have to regret that the points of difference are so numerous, and its degree so wide. The respect and admiration for his powers of chemical research, which all must feel who are devoted to physical science, are entertained by none more strongly than myself. But this renders fidelity the more needful. In proportion as his authority on other branches of science is deservedly high, and as in the work itself he has given valuable support to the most approved doctrines respecting animal heat,—those of Crawford, as modified by Lavoisier,—and has advanced opinions upon other points worthy of much attention;—in such proportion is physiological truth in danger of suffering by a too hasty adoption and publication, on his part, of any unsound views. Of such I am compelled to say there are not a few; and some of them of a character so obviously inadmissible, that I think we may entertain a satisfactory assurance they could not have passed into print, but in a work executed with greater despatch

* Appendix II.

than admitted of a cautious examination of each. Not to have commented upon such, when treating upon the very questions to which they relate, would have amounted to a virtual admission, while it would have been an unallowable omission, of them.

The VIEWS which form the substance, as well as the title of the present work, will be perceived by the physiological reader to be, for the most part, original; and it is for him to judge of their correctness and importance. The subjects themselves are abundantly important.

Part I. treats of the capacity of the chest, and the nature, condition, and duty of its gaseous contents, under the term *Statics*. For these points an importance is claimed, hitherto exclusively attached to the act, and the air, of respiration; and a considerable modification, therefore, of our previous opinions is demanded.

Part II. treats of animal heat, and contains various facts and arguments derived from observations made chiefly in India, which open to the mind fresh views upon certain questions connected with the development of heat, and the control of the vital powers over that process, and over the use and destination of the food. They also point to respired nitrogen as having another office to perform, than that of a mere diluent of oxygen. An early result of these observations was a conviction, that Lavoisier's hypothesis of the formation of water, as well as of carbonic acid, in the animal system, though of late years too generally discredited, would eventually be as firmly established as others propounded by that sagacious philosopher. I purposed, when the present work should be published, to contest, upon the grounds set forth in the sequel, for its re-adoption, and to dwell as now upon its merits. This I feel the more a duty, since the mention of Lavoisier, in connexion with that doctrine, has been omitted by Professor Liebig, whose main argument, respecting animal heat and the destination of the food, rests upon its admission. It is not to be doubted that this was an oversight on his part, since such mention was assuredly to have been expected in a work likely to be read by so many unacquainted with physiological history, or out of whose minds it had passed; especially where a theory had remained as in abeyance, and even been strongly opposed by recent and able authors.

This Part is divided into four heads; the questions discussed under which, and the views maintained upon them, invite a close examination.

Part III. treats of the diffusion and dissipation of animal heat. To the phenomena of its local abstraction, and retention, I would most particularly seek attention, since they direct us to questions of the first importance, — DETERMINATIONS OF BLOOD TO THE HEAD, —and point to an entire change in the views and prophylactic treatment to which we have been accustomed.

Although the chief object of the present volume is the advancement of physiology, the dependance of therapeutics upon that science is so close, that theoretical views in the one, will generally give rise to practical in the other. Hence several of this nature will be found interspersed throughout the former.

Of the practical views to which the physiological contained in this volume have given rise, certain have produced the gratifying results with which the action of the Respirator is associated. To many persons my pursuits are known chiefly in connexion with that instrument. It is therefore probable that some may open this volume with the view of finding a detailed account of its principle, use, and authenticated effects. This, however, would have required an entry upon various pathological questions, and upon others relating to climate, which, with a full detail of interesting cases, would have swollen the volume to a size, and given it a character, not intended; especially as I should not have considered the notice of the Respirator complete, without an examination of the objective, and otherwise mistaken, opinions respecting it, of unreflecting persons. Although these are being daily refuted by facts, it would be right to show wherein they are unsound. Such an account of this instrument I hope shortly to publish in a work upon Artificial Climates.

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PART I.

ON THE STATICS OF THE HUMAN CHEST.

I HAVE, for the following reasons, been here led to employ the word STATICS in a sense nearer to its etymological meaning, than to that which we are accustomed to attach to it, as a term denoting a branch of physics.

The reflective physiologist who directs his attention to all that is contained within the compass of the human chest, and to all the functions and properties of all that volume of animate and inanimate matter, cannot fail to perceive that, studied separately, the various contents of the chest offer to us but a partial view of their importance; and that, to approach to a due acquaintance with the purposes they serve, they must be studied collectively, as a compound of vital, chemical, and pneumatic operations, all acting in concert and harmony.

In the adult chest, in its healthy state, these various contents bear some certain proportion and relation to each other. There is some proportional quantity of the liquid and gaseous fluids which is proper, producing some particular degree of fulness and tension of the lungs, which is the average condition of the chest.

This proper volume, and proportional assemblage, of living and inanimate matters, with their functions and properties, when comprised under one term, might be named the CONDITION OF THE CHEST. But the word *condition* is commonly associated in the mind with something more limited and particular, especially when applied to any organs of the human body. We are then so familiarised with its employment to denote a state as to health or disease, that the medical use of the term is liable to be produced in the mind, if we speak of the condition of the chest. It would have been desirable to place the following matter under such a familiar head as "respiration," had not the process of respiration already so absorbed all attention, as to have prevented us from affording to the volumes of air durably resident in the chest the place due to their importance.

The word STATUS denotes also more exactly the condition of things before our view. As this is scarcely yet an English word, and since that balance of pressures, of which STATICS is the accepted term, is largely concerned in the maintenance of the condition of things which is our subject, I have thought it allowable to employ, as a comprehensive term by which to express this condition—THE STATICS OF THE HUMAN CHEST.

A dissertation on the statics of the human chest, extended into all their particulars, would occupy a bulk very much larger than that within which the present VIEWS are carefully limited. It is not a system or compilation of all that physiology and physics generally have made known to us, respecting our subject, which I have here undertaken. It is to certain particular points, which I conceive to be of the highest interest and importance, I invite attention in the following pages.

The first of these is the capacity of the chest; a subject, the experimental examinations of which having been very limited, and having as yet led to few theoretical or practical results, a field of inquiry offers itself, interesting in a physiological, and important in a medical point of view. The little attention it has received is perhaps an instance of the degree in which familiarity with it induces us to neglect a subject.

Every time a person yawns or sighs an experiment is unconsciously made, and a curious and important fact declares itself, inviting further attention than the mere consideration that the want of a freer circulation and oxydation of the blood in the vessels of the lungs causes the oppressive sensation which induces the sigh or yawn.

Without the aid of any nice apparatus to determine the quantities concerned, every person may, and often does unconsciously, make the following experiment. At the moment when he has completed an act of inspiration, or drawing in breath, and just before he performs the act of expiration or breathing out, he may, instead of the latter act, force himself to continue to inspire air, when he will find that his chest can take in, before it is distended, a quantity of air very much larger than that of any ordinary breath.

Again, after an act of expiration, at the moment when he would instinctively inspire, he may, instead of drawing in breath, continue to breathe out for a great length of time, if he is in health. If it has not occurred to him to make the experiment before, he will find to his surprise, that, at that period when the instinctive desire to take in breath had led him perhaps always to suppose his chest was empty, it still contained a vast quantity of air.

When, having expelled all this air, he continues to exert his utmost to empty his chest altogether, a limit will be set to his efforts before his chest is by any means emptied of air. The mechanism of the osseous fabric and muscles surrounding the chest is such, that the utmost motion they admit of is at an end, before all air is compressed out of the chest; there is, therefore, still a considerable quantity of air which cannot by any effort be expelled. This is the quantity which remains in the body after death.

To these several quantities we have to add the space occupied by the air of ordinary respiration. The constant motion of this air forces our attention to it, and renders us most familiar with its presence.

We have then before our view four distinct quantities, which it is necessary we should distinguish by certain terms by way of reference.

Commencing with the condition next to emptiness, namely, the bulk of air which we cannot expel, and which remains in the body after death, we may call this by a term which has been employed by others—the residual air. Then we have, on the top of this, the large bulk, which we can expel *after an ordinary out-breathing;* this may be named the *supplementary air*, it being the quantity filling the chest below the region of respiration. Upon this comes the ever-fluctuating air of respiration, which, in its influent state, may be known as "the fresh breath," and in its effluent state as "the stale breath." Over and above all this we have the capability of the chest to receive, when the fresh breath is already in, the occasional quantity which enters with a yawn or a sigh, and which may be termed *the complementary air*.

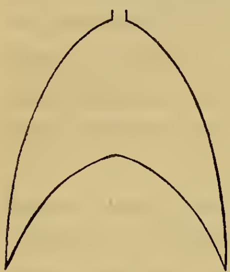
When we shall duly consider the importance of each of these several bulks of air, the fact must appear curious that it is the breath alone which is made the subject of speculation, and upon which the various theories of respiration up to the present hour are built.

Next, with respect to the bulk of these several quantities. That which is here termed the residual air, namely, the air which cannot be expelled by any voluntary act, or during life, has been variously estimated. We owe to Goodwyn many difficult experiments to ascertain the quantity. Considerable as is the merit of his essay, the experiments, from their nature, do not admit of our relying upon them; nor upon Mr. Coleman's, as they are described by Dr. Bostock, with whom we may safely conclude, that his estimate of the quantity was much too low. So, likewise, I conceive was that stated to me by a late eminent surgeon as the result of his experiments. As it is of no moment to the views before us to know its exact quantity, I shall not occupy space by discussing the defects of the several experiments. Some of these may be found well set forth in a work of the first merit.* The quantity, in fact, there is reason

* Bostock's Physiology, p. 316. to 320.

for believing, varies very much, not only with the size of the chest, but with its rigidity, and the habits of life of the individual. To name some quantity, we may take Dr. Bostock's estimate of 120 cubic inches. This we may consider the average amount of the residual air, or that occupying the air vessels of the chest in its most compressed state; which the outline (Fig. 1.) may serve to represent, though of course not a correct delineation of the chest.

Fig. 1.



We have next the supplementary air, or all that which can be expelled after an ordinary outbreathing. I have found this quantity to vary considerably in different persons. It appears to be affected, not only by the size and sex, but very much by the state of health, and by the habits of the individual; a fact, the importance of which will be presently considered.

It is very difficult to attain to accuracy in experiments upon the chest. Few persons, without considerable practice, can be brought to give a natural result. To this we may attribute so great extremes as those of Jurin, who estimated this supplementary air at 220 cubic inches, and of John Bell, who fixed it at only 70, while Fontana's estimate is less still. I may venture from my own inquiries, made with apparatus constructed to insure accuracy, to state that the average quantity, in men of middle age and size, exceeds 110 cubic inches. I judge the average, from many trials on persons of different sizes and habits, to be from 125 to 140 cubic inches. While in the bulk, the quantity is probably within a range of 20 inches above or below this, the *extreme* may, I think, range in different persons, even where no disease is present, from 80 to 180 cubic inches.

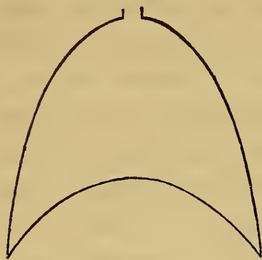
Jurin's *average* of 220 cubic inches is much too high, and Dr. Bostock's average of 160 or 170 is, I have no doubt, too high likewise, that being, as just observed, about the greatest extreme. On the other hand, Bell's is much too low. The quantity, however, of this supplementary air (or that air which can be breathed out still after an act of expiration), varies greatly, as already stated, in different persons, and in the same individual at different times. It is much lessened by a full meal, or by distension of the stomach by wind.* I have found it in an elderly female, who had long suffered from chronic bronchitis, to amount only to 40 or 45 cubic inches.† I consider this to be an extreme case, and think there must have been considerable inaccuracy in Mr. Thackrah's experiments, in which many working females are stated to have been able to expire 3½ pints only, especially if, as would appear, the air of respiration was included in this estimate.

Taking the average quantity of the supplementary air in men at 130 cubic inches, when this is added to the residual air in the chest, which cannot be expelled, and which has been taken at 120 cubic inches, we have a total of 250 inches as the aërial contents

* Appendix IX.

† I have not had the opportunity of trying the gaseous contents of the chest in any of those infatuated females whose lacing-up is excessive. In some of them the supplementary stock may possibly be under 40 cubic inches!

Fig. 2.



of the chest, at the time when, having breathed out, the instinctive desire to draw in air leads unobservant persons to suppose their chests are empty. The chest may now be represented by the outline fig. 2., when it has the two quantities of residual and supplementary air in it, or a volume of 250 cubic inches.

With respect to this, which I have ventured to name the supplementary air, Dr. Bostock, with good reason, expresses surprise at the fact of this large volume of air having been overlooked by several authors of superior merit, in their estimates of the gaseous contents of the chest. But the recognition only of its presence, unaccompanied by an inquiry into its use, failed to give it any importance; hence it has since been commonly lost sight of again. A late surgeon of the highest eminence, in relating to me his inquiries into the air contained in the chest after death (the first in the list above, and named the residual air), committed the same oversight, and spoke of the whole capacity of the chest as comprised in the residual air and the air of respiration; he having overlooked the second and fourth quantities, the supplementary and complementary air, of our list. Again, in a recent treatise on physiology, of great merit, the very same oversight is made; and in the still later publication of Professor Liebig, devoted especially to that part of physiology which embraces the function of respiration, the exclusive attention he affords to the air of respiration leads him into statements which I am compelled to pronounce incorrect in themselves, and involving in them a contradiction with many other portions of the professor's work.*

When these two quantities, then, the residual and supplementary air, are in the chest, we arrive at the region of respiration, the air constantly flowing in and out, and known as "the breath." It is this air to which almost all inquiry has been directed. The experiments of different inquirers into its quantity differ greatly, varying no less than from 10 to 50 cubic inches. Upon numerous trials carefully made, I have satisfied myself that the difference does actually vary greatly, not only in different persons, but in the same person at different times.*

Physiological reasons, which will appear in the sequel, led me indeed to this conclusion long prior to its confirmation by experiments; while, however, the difference in the bulk of the breath is, in different cases, very considerable, there is no such extreme as in the averages of different experimentalists: 10 cubic inches are far too low, and 50 too high an estimate; perhaps no experiments have been made with greater pains than those of Dr. Menzies, yet I cannot doubt of his having fixed the quantity too high, and a trial which he mentions as confirming his estimate, appears to me to assure us that it was too large. Having by a previous apparatus arrived at the conclusion that the volume of the breath averaged 40 cubic inches, he placed the subject of his experiments in a bath up to the neck, and

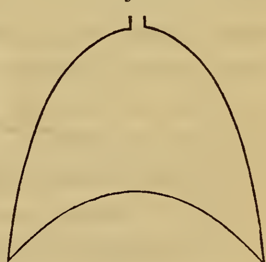
* Appendix I.

measured the quantity of water which was displaced by the process of respiration: this proved to be 40 cubic inches, and has been considered as strongly corroborative of the estimate. I find, however, that under such circumstances the breathing is rendered more laborious than natural; the intercostal muscles act more than when there is no heavy column of fluid pressing on the abdomen, and checking the free action of the diaphragm. When water is not so cold as to produce any quick panting action of the diaphragm, its weight causes this muscle to be less ready in alternating its contraction and relaxation: when it has commenced to contract against a resistance, as that of the body of water, it continues the action longer, as if fearing to give way; the movements are slower, but more extensive than usual. In a case where the average bulk of the respiration is 26 cubic inches, I find it to rise to 40 or even 42, when rendered laborious; hence I feel justified in attributing Dr. Menzies' large estimate to this cause. On the other hand, not only is the estimate of 10 cubic inches too low, but that also which has of late years been pretty generally adopted, 20 or 22 cubic inches. The real quantity is so various and variable, that a single average can hardly be named. I find the *average* of unembarrassed respiration in full grown men to range between 24 and 32 cubic inches. In one case I found it so low as 16, while in some I have no doubt it may amount to 40; but I conceive this to be rarely the *average* of any person, though, when he is excited or under exertion, it will readily mount up to 40, in a person whose average is only 25 cubic inches; and there can be little doubt that the volume must be constantly varying with the need of the system. Active exercise also, and labour, conduce to a habitually fuller respiration; physiological reasons for these variations will be noticed hereafter. But no causes tend to increase its bulk so much as a residence on lofty mountain heights *: during such a residence on the Himalaya mountains, I could perceive this effect very plainly.

If a single average is to be named, I conceive that 26 cubic inches are a fair mean, and that from 19 to 20 of such respirations will be made in a minute. Hence a volume of about 500 cubic inches of air enters the chest in a minute; but in the same person there is little doubt that at certain times it will not be more than 400, while at others it may rise to 600, or more.

When the air of the breath is added to the preceding quantities, that is, when a person has drawn breath, we have 250 cubic inches of residual and supplementary air, and 26 of air of respiration, making together 276. The chest being then somewhat larger, may be delineated by the outline fig. 3.

Fig. 3.

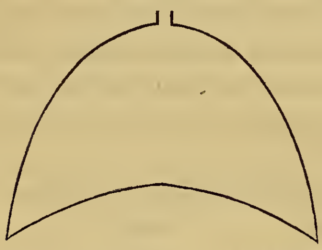


Lastly, we have to estimate the complementary quantity, or that volume which will fill the chest to its fullest state of distension over and above the quantity present after an ordinary breath has been drawn. The quantity of this, likewise, varies much. Its

* Appendix I.

volume depends greatly upon that of the supplementary, bearing an inverse proportion to the latter; that is, when the one is large, the other will be proportionally small. I am not aware of any prior experiments directed to this volume of the chest. After several trials, I am led to consider the average proportion of the complementary space to the suppletary to be about four fifths of the latter; but in some cases of vigorous persons, it probably does not exceed one half of the supplementary, while in others it is larger than the latter. Taking the average in healthy, but not robust men, at about four fifths of the supplementary, we may fix it at 100 cubic inches.—Fig. 4. may serve to represent the chest when distended with this complementary air. It will now contain the four volumes as follow:

Fig. 4.



	Cubic Inches.
Residual air - - - - -	120
Supplementary ditto - - -	130
The Breath - - - - -	26
Complementary air - - - -	100
	376

Of all these quantities, the air of respiration, though much the smallest, has alone engrossed attention. In the visible motion of the chest produced by its influx and efflux, and in the perceptible current from it at the orifice of the mouth and nostrils, together with the manifest and immediate connexion of the breath with life, we find enough to command the attention of the most uninformed. This air does indeed possess a higher importance, and claim the first consideration in our physiological inquiries, but the exclusive attention it has received affords ground for regret, as I think will appear, when we pursue our inquiry into the uses of the other volumes of air present.

Let us now direct our attention to the first and second volumes of air in the preceding table, namely, the residual and supplementary, and we shall find them to offer to our view facts of much physiological interest, and of no small medical importance.

In the first place, these volumes of air are present when the chest is emptied of the ordinary breath, or air of respiration; that is, after we have done breathing out there remains permanently in the chest a large quantity of air, estimated above at 250 cubic inches, supplementary and residual taken together. These two volumes, both with respect to their use and to their locality, may be considered as one, and as they are permanently resident in the chest, may be comprised together as *the resident air*.

The resident air, then, we may understand to comprise the residual and supplementary air, and it is that large quantity of air always in the chest, over which the air of respiration is introduced in breathing; this latter air amounting only to 24 or 30 cubic inches, and being, of course, when it is inhaled, pure atmospheric air.

As already noticed, the air of respiration has absorbed the attention of chemical physiologists exclusively; all their inquiries have been directed to it. It is spoken of as acting upon the blood in the air cells, and, when breathed out, as having given up a portion of its oxygen to the blood, and having received from the blood carbonic acid and watery vapour.

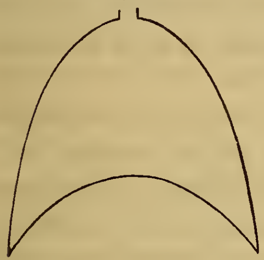
So familiar is the mind with this view as an established fact, that the announcement of the following as the correct view may appear an act of some temerity, but the truth of it may be readily and indisputably proved, while it points to others of much interest and importance.

We may affirm, that the air of respiration does not, and cannot, ever enter the air cells, nor even the smaller air tubes. It has no *direct* concern in the oxydation of the blood, nor does it receive its carbonic acid and vapour directly from the blood. Furthermore, its constant presence in the air cells would be injurious to health, and, probably, soon fatal to life. The air of the breath has, in fact, no business with the blood, nor any footing in the cells of the lungs.

The truth of this will at once appear, if we duly reflect upon the structure of the lungs, and the position which the *resident air* in them occupies.

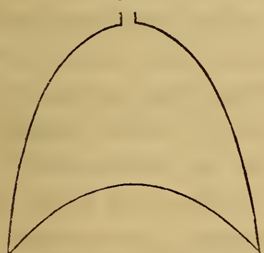
We have first to bear in mind that the chest is always full of air up to the throat itself. It is so, even when, in common language, we speak of it as empty of air. This fact is obvious to every pneumatician, who knows that it cannot be otherwise with a vessel like the chest, having comparatively delicate walls, under such a gaseous pressure as that of the atmosphere. The compressibility of the chest and abdomen, and the open state of the windpipe, insure that there shall be gaseous fluid filling all free space in the chest, of exactly the same tension or elasticity as that of the air without.

Fig. 2.



Reverting, then, to fig. 2., representing the chest contracted, by breathing out, so far as to have the air of respiration expelled, and to contain now only the *resident air*, it must be plain, when, upon an instinctive expansion of the chest, air enters by the windpipe, that it is the pressure of the atmosphere which sends it in, and that this air must press before it the whole body of resident air. The chest now assumes

Fig. 3.



the somewhat larger dimension of fig. 3., having the 20 or 30 cubic inches of *breath* added to the 250 of *resident air*.

Were the lungs a simple hollow cavity like a bladder, or like one of the figures 2. or 3., it must be obvious that the air of respiration, coming in upon the other large volumes, could not, by any means, arrive at the bottom of such lungs, without mixing itself with the resident air.

But the actual structure of the lungs is such, that it is not possible the air of respiration should even do this. It cannot even mix itself with the whole resident air: the utmost it can do is to mix itself with the uppermost strata of the resident air occupying the larger tubes. There are no principles in pneumatics which admit of any other view.

On the expansion of the chest at the moment of breathing in, the *resident air* evidently must and does recede before the entering breath, the portion of it most distant from the breath adding itself to the resident air already in the cells, so as to fill them up as the chest expands.

There are no forces in action which should make the *entering* air take the place of the resident air, and descend into the cells. There is no difference of gravity arising from the entering being the colder air, which would have this effect, for by the time the inward breath has reached the chest, it is, we are sure, warmed up to within so few degrees of the resident air beyond it, as to have any superior weight remaining from this cause more than counterbalanced by the considerable quantity of weighty carbonic acid in the resident air. There is not, in fact, any gravitating tendency to cause the new, to descend and press up the resident air; and it would require some decided power, superior to any trifling difference of gravity, did this exist, to force the fluids so instantaneously to exchange position; to do which they would have to pass by each other in a multitude of minute tubes, an action, the existence of which we cannot entertain for a moment. Lastly, they are not like oil and water; they could not pass without an immediate commixture taking place in the upper and large branches, long before the *breath* reached even the smaller tubes.

But any reflecting person, having any acquaintance with pneumatics or hydraulics, cannot fail to discover the fact, directly it is pointed out, that the breath, at each entry, can only mix itself with the uppermost portions of the resident air immediately contiguous to it.

An indraught of breath of 26 cubic inches may mix itself, during the respiring act which brought it in, with 60 or at most 80 cubic inches of the resident air. Hence it follows, that when a corresponding quantity of air is breathed out, it is not that which went in, but a mixture of that air with the uppermost resident air.

If we allow that the mixture can have reached so far as to involve the upper 80 cubic inches of the resident air, the air breathed out will obviously consist of one fourth of atmospheric air, and three fourths of resident air.

We here perceive, that the air breathed out is not the air just before breathed in, but a mixture of which *it* can form a part only.

Fig. 5.

It may be asked, how then are the purposes of respiration fulfilled, and how are the smaller air tubes and cells supplied with air sufficiently oxygenous?

Two ways offer themselves, both of which we must admit to be concerned in the process.

The first and chief is that of a progressive intermixture, proceeding by steps from without inwards at each inspiration. Let fig. 5. represent any one ramifying air tube, the extremity *a* being that next the mouth, and *e* the termination in air cells; and let the portion of the tube between *a* and *b* be that occupied by fresh air just drawn in. The portion between *b*, *c*, will contain a mixture of the previous indraught with the upper portion of the resident air; *c*, *d*, will be occupied with a commixture of this mixture with resident air below it; and lastly, *d*, *e*, will be a mixture in third degree, namely, of the second mixture with the resident air in the innermost part of the lungs. In this way is oxygenous air carried onwards, step by step, to the air-cells.

The other way by which the object must be aided, is that of the expansion of a gas out of its own mixture into any contiguous volume of air, in which that gas is deficient,—a process of the nature of endosmosis. Thus oxygen may travel down, and carbonic acid up, although the other gases in which they are do not change places. This last must necessarily be a gradual process in tubes of so small calibre.

By these two ways must oxygenous air be carried inwards, and the impure air return upwards, namely, by a progressive mixture of successive volumes of air from without inwards, and by penetration of oxygen downwards and of carbonic acid upwards, through the respectively dissimilar mixtures. In this manner, and in no other, can oxygenous air find its way to the cells.

But some will say, by such an arrangement the air-cells would never be visited by air of the freshness requisite for duly oxydating the blood. The reply to this is, that, whatever may be our preconceived notions respecting the presence of fresh air in the cells, the statics of the case render it impossible it should ever be there under ordinary circumstances. They assure us, beyond the possibility of a doubt, that it is *resident air* only, which moves into and out of the cells in the action of the chest. It is this resident air which performs all the duty of oxydating the blood, and which receives from the blood its eliminated carbonic acid, and watery vapour. The air of respiration performs no direct duty in connexion with the blood. In its fresh state it does not come even near to the cells; its duty is altogether indirect; its action is to ventilate the chest gradually, from above downwards, and to receive the impurities gradually brought up from below, exchanged for an equal bulk of more recent air, conveyed, in the manner described, from above.

Hence, if we examine the air of an outgoing breath, we err in supposing that the difference between the composition of it and the previous indraught is a change that has been brought upon the latter by the blood. The loss of oxygen by the one, and the gain of carbonic acid by the other, do not by any means inform us of the action going on in the cells, at the particular moment of time during which the breath was in the chest. All we learn from it is, the average change wrought upon a given considerable body of air in a given time.

It is true, indeed, that the indications are, if the experiment be carefully made and continued upon several breaths, the same as if the fresh air of the breath were itself acted on by the blood; but the above fact renders the necessity of care in regulating our respiration during the experiment very necessary. The results are at once vitiated, if we respire at all more deeply than usual, or allow an undue quantity of the resident air to be expelled.

When this fact is properly considered, it will explain the very different results of different experimentalists, and may lead us to desire still further trials, conducted with the above view of the case kept well in sight. Until the fact holds a due position in the mind, that the *resident* air both forms the chief bulk of the contents of the chest, and is that which is wholly concerned in the oxydation of the blood,— until this fact forms the basis of his views, the chemical physiologist cannot have his mind in the best position for separating with care the resident air from the breath.

Some years ago I dwelt upon this point in papers published in the "Medical Gazette*," though I reserved a fuller exposition of the view until the publication of the present matter.

It has, indeed, been noticed in the experiments of some able inquirers, that in proportion as a deeper respiration was practised, the expired air contained a larger quantity of carbonic acid; but in most instances the paramount importance has not been kept sufficiently in view, of extreme care to confine the respiration during the experiment within the limits of ordinary breathing, so as not to involve in it any resident air. Hence the estimate of the deoxydation of the breath has been affected by properties belonging to the resident air, and consequently, I believe, too high. The experiments of Doctor Prout in 1814 confirm this view, and settle the proportion of carbonic acid at the mean of $3\frac{1}{2}$ per cent. in the expired breath, instead of 8 per cent., the estimate of others.

The recent experiments of Mr. Coathupe, which

* 1838

appear to have been made with much care, establish this very point; and I shall hope to offer a series of experiments of my own at a future time, in as complete a form as I would wish them to possess for publication. This subject will be recurred to in its proper place, the oxydation of the blood; where I shall take occasion to express the belief that much more carbonic acid passes off by the skin than is commonly supposed, and that the discharge by the lungs is so far lessened.

With respect, then, to the duty of the resident air, we find it to be the source from which the blood derives its oxygen, and into which it exhales its carbonic acid and vapour.

A question naturally presents itself, why should such an arrangement exist, and why does not the fresh air of the breath, as we have been wont to suppose, bring its free supply of oxygen to the cells, and oxydate the blood directly?

The advantage of the arrangement will readily appear. According to the usual view, it was necessary to suppose (if we attended at all closely to the process) that the action on the blood must be constantly fluctuating, being at a minimum at the end of an expiration, and increasing to a maximum during inspiration. Such, according to the common view, must have been the case. But was there supposed to be any fluctuating, or tidal movement of the blood in the pulmonary vessels, making the quantity of it present to correspond with so very varying a supply of oxygenous air? Our knowledge of the circulation forbids our entertaining any such an opinion. We know, indeed, that the diminution of pressure on the chest during inspiration, and increase of pressure during expiration, arising from the action of the diaphragm, &c., have, as hydraulics might assure us they would, an effect on the current of the circulation just perceptible; but we know that there is no such sudden and large flow of blood into the lungs at one moment, and cessation at another, as would correspond with the extreme ebbing and flowing of the breath. The inquiry, in fact, does not appear to have been pressed so far home.

We now, surely, may perceive the beauty of the arrangement which lodges a large quantity of air durably resident in the cells; which is there during expiration as well as inspiration, and also at the interval between the two; and is, therefore, carrying on its barter with the blood, at all times, continuously and uninterruptedly.

While this is going on, the process of *respiration* is alternately pumping in fresh air, and pumping out a mixed air; thus ventilating the chest from above, and keeping the resident air of some standard quality. Experiment tells us, this standard contains upwards of 8 per cent. of carbonic acid. The quantity is probably not under 10 per cent. Of this, presently.

In order that the action upon the blood should proceed equably and uninterruptedly, we now see the necessity of the *resident air* being large in quantity, and the fluctuating air, the breath, much less. The case may, to a certain extent, be compared to superior double bellows, such as those of an organ, the larger portion of which is kept in a steady degree of fulness by the quickly pumping movement of the other. The bellows-man knows well that, in order to have a very steady blast, he must keep a large quantity of resident air always in his bellows.

The next question is, how can we do such violence to convictions in favour of fresh air, derived, not only from preconceived notions, but from our sensations also, as to believe that no better air visits the cells of the lungs, than such as is charged with carbonic acid to the extent of 8 or 10 per cent.? and can we think such air sufficiently oxygenous for carrying on the active transactions with the blood which we perceive to take place?

Put in this way, the question may assume a serious aspect; but it will not be difficult to meet it—first with an unanswerable, and next with a satisfactory reply.

In the first place, however opposed to existing impressions, such is the statical position, and the relative quantities of the contents of the chest, and such the structure of the lungs, that the doctrine propounded has nothing hypothetical in it. It is a physical truth—an absolute and inevitable result of the existing data.

We must, therefore, bear in mind, that whatever difficulties might present themselves in the way of a satisfactory accommodation of it with other points, we have no choice left us, but to modify them to suit the fact propounded. To do this will not, I think, be difficult.

In the second place, then, with respect to the importance of having pure air to breathe, of which we are assured both by instinct and experience;—undoubtedly we cannot have too pure air. If its condition as to warmth and moisture (which I would call its temperament) be suitable, we hardly know a limit to the extent to which it is desirable to renew the atmosphere around us. Carefully observed facts connected with human life may teach us this; and so far from really militating (in vain it would be) against our impregnable position, it will, on due examination, be found, I conceive, to support it.

Since the ventilation of the innermost parts, the air-cells, of the lungs does not take place, out and out, at each act of respiration, but gradually and indirectly, through a change progressively wrought on the bulky *resident air*,—since the fresh air, in short, forms so small a part of the whole, and can mix only with the uppermost portions of the resident air,—since such are the conditions of the case, is it not all the more desirable that what does enter should be very fresh? Hence the jealous sensibility of the orifices of the mouth and nostrils, especially of the latter and of the larynx, to impurity of air. Hence to these portals to the lungs the care is confided, by investing them with a watchful sensibility, of seeking the purest air that can be had for the ventilating process. But as we go deeper, we find, as a matter of fact, that this sensibility gradually decreases. In the windpipe it is less than in the larynx, and in the bronchi less still.

In this decreasing sensibility, we perceive a corroboration of the *view* here taken. The *statical fact itself* is a truth too absolute to admit of corroboration. Thus nature having so arranged matters, that of the whole air (the resident, and the tidal or respired), that which is at the summit of the lungs shall be quite pure; and that the air shall gradually decrease in purity, until that which is in the cells is charged with 8 or even 10 ten per cent. of carbonic acid : such being the natural arrangement, the successive surfaces are given a corresponding degree of sensibility. Thus we are very comfortable while we have our air-cells occupied with air so highly carbonated ; it occasions no uneasiness through occupying the greater part of the lungs ; but were similarly impure air to be uninterruptedly present at our nostrils during inspiration and expiration, we should turn from it with feelings of suffocation.

We have yet to meet that which appears to be the most serious and objective part of the question; namely, why should such impure air be always resident in the lungs, and how can we entrust to it the active duty of oxydating and defecating the blood?

Let us examine the latter portion of this question first. It were very unphilosophical to allow our minds to be occupied by preconceived notions, and to assume as absolute any thing respecting the qualities proper to be possessed by the air in the cells, but what we learn from existing facts. We cannot say from any other known data whether air, at the time it is acting on the blood, should contain 10 or 20 per cent. of oxygen. Our business is to ascertain what are the existing facts of the case, and what is actually the usual quality of the air in the cells. This we have seen is not atmospheric air having 21 per cent. of oxygen, but resident air with, at that depth in the chest, probably about 10 per cent.

Such being the *fact*, arranged by the Creator, we may be well assured that it is *right*, and we must therefore conclude that it is quite an error to think that *atmospheric* air ought to be presented to the blood; and that somewhere near 10 per cent. is all the oxygen it is proper for air to contain, which is to act directly on the blood.

Such being the fact, we may discern in it a beautiful provision, offering an answer to the other portion of the question—why should such impure air be always resident in the lungs?

Is the following not a very satisfactory reply?

As we proceed from the larger air-tubes onwards through their numerous ramifications, till we are lost in searching out the delicate cells, do we not find the pulmonary membrane lining the way, commencing comparatively thick and tough, and getting finer and finer, until at last it becomes too delicate to be clearly discovered, a mere film, overspread by equally delicate blood-vessels? Again, though the greater part of the business of oxydating the blood appears to be carried on in the cells, we are not to suppose that the extensive surface of membrane expanded over the lengthened and infinitely numerous tubes leading to the cells is unemployed. Such a view does not accord with the economy of means every where discernible in the body, and it is opposed to the observed development of the blood-vessels, which travel along with the tubes, and spread their minute branches over them, in the same way as, at the extremity of their course, they do over the cells.

There can be no doubt that, in tubes where the pulmonary membrane grows thin enough, there the air begins to penetrate through it, and to act on the blood circulating over such tubes. Let us suppose the action proceeds with due activity at some given distance in the lungs, where the pulmonary membrane has a certain thickness, and the air in the tubes a certain per-centage, say 18. If such a proportion of oxygen acts with *due activity* through a membrane of such a given thickness, could we refuse assent to the probability, (were it not a fact absolute,) that, as the membrane grew more and more delicate, less and less oxygen should be found in the air, until in the cells the proportion of oxygen should be reduced so far as to guard against injurious activity in the process, where an infinitely delicate membrane only was interposed between the air and the minute blood-vessels? Assuredly, if, where the membrane was *much thicker*, the process went on with *due activity*, its activity would become *far above what was due*, when the membrane became of extreme tenuity, unless the quantity of oxygen in the air fell in proportion, unless the air became as it were diluted in proportion.

We must either suppose that it is only in the cells, where the membrane is in the very utmost state of tenuity, that the action between the air and the blood proceeds with any due activity, which, judging from the operations of nature, and the development of the smaller pulmonary blood-vessels, is inadmissible, or we must admit that air which could act properly through the membrane where it was not so thin, would act too powerfully where the tenuity of the membrane, as in the cells, was extreme.

If this be so, we may perceive a beautiful and harmonious arrangement accommodating the oxygenous strength of the air to the resistance of the medium it has to penetrate. Should the reader not feel as satisfied of the truth of this explanation as the writer, he must still bear in mind that the rejection of the explanation of the *use* of the arrangement does not at all affect the *fact* of the existence of the arrangement, which is not to be controverted.

The points which have been above established with respect to the position occupied, and the offices performed by the resident air, and the air of respiration, (whatever we may think of the *reason* just assigned for the arrangement,) these several points, established beyond a question, direct us to many practical truths of great importance, which may now be considered.

It is a frequent ground of surprise that so delicate an organ as the lungs can endure, when healthy, to receive into it air, not only very trying, on account of extreme coldness or dryness, but even very irritating from dust or smoke. Even with the clear view of the statical condition of the chest before us, which such an inquiry as the present affords, the fact continues worthy of surprise; but if such irritating air really visited the most delicate part of the lungs, especially the cells, as is commonly supposed of the breath, we may be assured that it would be productive of serious injury to the strongest persons.

The truth evidently is this, that since the air of respiration, as such, penetrates but a small way, and since it can only work its way down to the deeper part of the lungs by repeatedly mixing with successive portions of the bulky resident air, any irritating properties it possesses must become greatly diminished by the repeated dilution of it, before any portion of it can reach the cells. We may satisfy ourselves experimentally of the truth of this by breathing out to the utmost, so as to expel as much of the resident air as we can, namely, its supplementary portion, and then, drawing in a dusty or otherwise irritating atmosphere, we shall be immediately distressed by it, though we might have been performing ordinary respiration in it without difficulty.

But for this provision of a large body of resident air, it is probable that, when the membrane was in an inflamed state, it would be impossible to respire air of ordinary dryness, such as that in the ward of an hospital. In papers recently published in the "Medical Gazette*" I have endeavoured to set forth the great importance in such affections of providing, and administering *uninterruptedly to the lungs alone*, an atmosphere of a suitable *temperament*, i. e. of a condition as to moisture and temperature, which shall as certainly tend to soothe the irritated membrane, as does a fomentation or poultice an outer inflamed surface. That the air respired throughout the acute stages of such diseases is, on account of its ordinary dryness, a cause of a great deal of the distress and obstinacy which is their characteristic, I cannot doubt. The wonder is, how the irritation ever subsides under the constant influence of air from which we would have scrupulously to guard an inflamed eye, if we would save it from destruction. Serious as must be, I conceive, the irritating effects of ordinary air to an inflamed chest, there can be no doubt that it would be insufferable and destructive, if, in the process of respiration, it were being con-

* 1842. No. 22. to 53. inclusive.

stantly drawn into the extreme parts of the lungs, and swept the very air-cells.

The panting and short respiration in such cases shows that even its ingress to the natural extent cannot be endured.

In the cases of pulmonary irritation, then, we may see the same advantage as in health, only in a greater degree, of the wise provision which does not permit irritating air from arriving undiluted at the more distant parts of the lungs. Even with this guarding arrangement, to how many persons is the respiration of air of various natural and artificial qualities distressing!

The next point for consideration is the particular place or region which the act of respiration, the ebb and flow of the breath, occupies in the wide range of the chest's capacity.

Reverting to former pages, we find the fact dwelt on, which is obvious to every person directly it is attended to, that the act of respiration is performed when the chest is in a certain stage of fulness or partial distension. The breath flows in and out on the top of a large quantity of air (the *supplementary* and *residual* air classed together as *resident air*), and within the limits of a large range yet remaining, which I have named the complementary space. A space of course it is not, until it is realized through the expansion of the chest by a very deep indraught of air. Yet it is at all times (in health) available, and the air it can contain I have called the complementary air.

Now, we have to consider the use of this arrangement, — why the region of respiration is placed, as it were, two thirds of the way up the chest, and whether it occupies the same locality in all persons, or the same in the same person at different periods.

We might conceive matters so arranged that respiration should take place at the top of all, upon the chest full, the complementary being added to the supplementary and residual air. Or, on the other hand, it might have been arranged that the breath should flow in and out when the chest was emptied as far as possible, that is, with only the *residual* air in it, which cannot be expelled. We can, indeed, force ourselves to practise respiration for a short time with the chest in either of these extreme conditions.

We have already seen abundant reason for the arrangement which places a large quantity of *supplementary* air at all times in the chest, over and above the residual air, since it guards the delicate parts of the lungs from the visitation of air of irritating qualities. This is one obvious and unquestionable use of the arrangement. We have reason also to believe the primary and main use is to allow only air having the diminished quantity of 10 per cent. of oxygen to present itself to the membrane of extreme tenuity lining the cells, lest the action on the blood should be too active; and if we will not admit this reason, we may remain satisfied without one; but we cannot destroy the existing facts.

To these reasons may be added one which is sufficiently evident to strike any person. It must be desirable at all times, after expiration as well as after inspiration, that the cells and tubes should be at least partially dilated, in order that the blood in the vessels ramified over them may flow freely. Were the cells to be constantly emptying themselves, and their cavities closing up, the vessels spread over them would, at such times, be doubled, folded, and compressed together, so as to have the circulation in them materially obstructed.

Such, then, appear to be the chief reasons why we do not respire as it were at the bottom of our chests, upon empty lungs; and they show the arrangement to be one serving many purposes in admirable harmony with each other.

With respect to the reason why we do not breathe as it were at the summit of our chests, with the lungs already filled with the complementary as well as the resident air, it must be sought in the fluctuating demand for air of respiration.

During exercise, and especially during considerable exertion, we know that the hurried circulation of blood through the lungs calls for a more copious supply of air. To command a range for a deeper respiration, we must either breathe out some of the resident air, and add the room thus gained to the previous range of the respiration; or, retaining in our chests the same quantity of resident air, we must increase the respiratory range by intruding upon the complemental space.

This is no trifling distinction. What is vulgarly termed "being in breath," and its opposite "not in breath," appears mainly to depend upon these different modes of increasing our respiration. An unpractised runner, for instance, tries to relieve himself by the former method; but he soon feels the consequence of letting out too much of his resident air, and drawing in too deeply atmospheric air, fully oxygenous, and perhaps also cold. He gets out of breath — that is, when he wants more air than usual, he cannot take in so much; a kind of asthmatic spasm prevents him from getting air enough down, although the chest is not really much more than half full. On the other hand, by practice he instinctively learns to keep adding air to that already present, and to breathe nearer to the top of his chest. He can then respire deeply without drawing in the fresh air too suddenly and too far into the lungs. Also, by increasing the quantity of resident air, his cells are more fully expanded, there is more surface of action, and the blood-vessels are rendered less tortuous still, by which they admit, with less distress, of the quickened circulation through them.

So likewise, when from drowsiness or languor, the respiration has been proceeding feebly, we are led by the oppression thus occasioned to yawn; that is, to gain a large quantity of fresh air by a deep sonorous act of respiration, which air goes in to supply the lack of oxygen, and to wash out, as it were, in the following long expiration, a portion of the resident air, which had become too little oxygenous during the languid respiration. Now we shall find upon attending to the act, that in order to get this air we do not begin by breathing out, as it might be expected we should, in order to clear the chest of impure air, and give a deeper entrance to the body of fresh air we are seeking; but we commence by breathing in to the utmost extent we can; we retain the air some time in the chest, and then breathe out a quantity equal to that we draw in. The reason of this appears plain. It is trying to the lungs, rather than a source of comfort, to expel the resident air to a large extent, and then draw in fresh air deeply. If a large draught of air is taken, it must be received upon a large bulk already present. It is then prevented by this bulk of air from penetrating too deeply. We find, then, the action of yawning, as it is actually performed, corresponds with all the other views we have taken. The act of sighing differs from yawning chiefly in the effort being not only to give air to the chest, but to give freer motion also to the circulation in the lungs, which had been retarded by some depressing cause acting through the mind. In sighing, as well as in yawning, the large indraught of air takes place upon the top of the body of air already present, and, no doubt, for the same reason.

We have now seen good reasons for the natural arrangement which places the region of respiration neither at the bottom nor at the summit of the chest, but about two thirds of the way up towards its full state. The remainder of the question has to be considered, whether this locality or region of the respiration corresponds in all persons; that is, whether the space below and above it (the supplementary and complementary space) bear an equal proportion to each other in all persons; also whether in the same person the region of respiration is the same at all times.

These are more important inquiries than may at first sight appear. There is strong reason for believing that the region of respiration differs in different persons; all my own experiments tend to this conclusion; and that, very generally, in proportion as a person is vigorous, his breathing is established high up the chest (if the expression may be used), that is, he retains a large quantity of supplementary air in the lungs; he is then able to take fuller and freer draughts of air without exciting a tendency to cough or a feeling of tightness. Muscular exertion tends greatly to establish a permanently fuller state of the chest. The extent to which the chief muscles of the trunk of the body are inserted into, or have their origin from the walls of the chest, is one cause of this. In order that such muscles should act with power, we have to draw in a larger quantity of air than usual, and when we want to make a considerable effort, as in lifting a heavy weight, we have to close the windpipe and detain all this air in the chest. The walls of the chest, the ribs, &c., then are stiffly supported by this bed of air, like a distended bladder, or air cushion. In this way, the chest can support a great pressure, and forms a firm basis for the vigorous action of the muscles attached to it. When longer continued, but not so strenuous efforts are made, as in carrying a more moderate weight for some distance, and even in active walking without any load, a man still keeps his chest more than usually distended; holding the air in for a time exceeding the period of an ordinary breath, and then letting it out to take in a fresh stock of complementary air (to use the term adopted), to give stiffness to his chest.

Now this action being frequently repeated, must and does have the effect of establishing a permanently fuller state of the chest. It is, in fact, the rendering a person "broad-chested;" the connection of which with vigour is too striking to be overlooked even by the uninformed, who do not fail to see the fuller condition of the chest, though without an acquaintance with the manner in which it is brought about, or in which it is advantageous.

In such vigorous persons, then, the supplementary air becomes larger, a portion of the complementary space being added to it, and then ordinary respiration takes place on the top of this increased supplementary quantity. That this is true, we may satisfy ourselves by measuring the quantity of air such a person can breathe out, and comparing it with that breathed out by a person of sedentary habits. We shall find that the volume of the air durably resident in the chest is much larger in the former, the comparison being made between two persons of the same bulk.

Having a larger quantity of resident air always present, such persons can, as already explained, draw in upon it a deeper breath without the fresh atmospheric air penetrating to a region where the pulmonary membrane is unfitted for the presence of air fully oxygeniferous, and also having other qualities arising from its temperament, and from mechanical impurities, as dust, which are especially irritating to the more delicate parts of the lungs.

On the other hand, they whose misfortune it is to lead a sedentary life, and to lean over their work, habituate themselves, by the constant doubling together of the trunk, to do with a smaller quantity of resident air in their chests than is natural, or proper. In them, then, the air of respiration is at once introduced to a deeper region of the lungs than it ought. Though it is impossible, in any case, to exist with so little resident air in the chest as that the air of the breath should flow unmixed into the air cells themselves —for the residual air which cannot be expelled is bulky enough to dilute it considerably— yet, when the quantity of resident air is materially reduced, it is plain the air of the breath goes in too far, and proves exciting to tubes too delicate to receive it, on account of its full quantity of oxygen, and also, no doubt, of its *temperament* and other qualities.*

* In a series of papers on artificial climates, recently published in the "London Medical Gazette," (1842, No. 22. to 53.,) I had occasion to mention a fact, in connexion with the well-known suffering of the grinders of cutlery in Sheffield, which I learned when on a visit to that town. In the hope of providing a remedy for an evil which reduces the span of life by twenty years, I visited some of the larger works. I found that stone, as well as steel dust, was evidently concerned in doing mischief to the lungs of the grinders, *and that the mischief was greatly aggravated by the bent position of their bodies.* This the men mentioned of their own accord, as a matter of familiar observation. Even in the few cases where, by effective air machines, the whole dust was drawn away, the lungs still suffered, though in a diminished degree. The body was so doubled forwards, that the organs of the abdomen were pushed up under the ribs, intruding into the space belonging to the chest. In these poor men the *resident* air was greatly diminished, being deprived of the greater part of its *supplementary* quantity, the *residual* air being, to appearance, nearly all it amounted to. Even if the air they breathed were perfectly free from impurities, it could not, I conceive, fail to be soon destructive, by finding much too small a quantity of resident air in the lungs to receive and dilute it. Hence its full quantity of oxygen, its temperature, and its dryness, however proper for lungs ordinarily full of resident air, were all combined in doing injury, when it was not properly diluted in the

The distress which the presence of pure air produces in tubes intended to receive only mixed air leads such persons to accustom themselves to do with less breath than is natural. It is quite an error to think that their chests, at the time, will not contain more breath, on account of the position; for if they were to breathe out still more of the resident air, they might leave more room for breath than the volume of the breath ever requires, and yet keep their chests within the confined limits they had been reduced to. The truth of this may be noticed, whenever a medical man, or friend, remonstrates with a girl on account of her tight lacing. One whose folly has nearly reduced her figure to that of an *insect*, and whose countenance betrays the state of her lungs, will yet be able to show that her stays are "quite loose," by thrusting her hand between them and her body. Many a friend is deceived, as well as the

lungs. When to these was added the sharp dust, with also, perhaps, the chemical action of the steel, the evil was prodigiously increased. There was not even the partial protection of a large bed of resident air to receive the air loaded with these irritating particles, and in some degree to ward it off. In addition to all this, there is a constant effort in such a case to do with as little breath as possible. The consequence of this is an imperfect oxydation of the blood and all its attendant evils. Also the compressed state of the lungs necessarily prevented a proper circulation of blood through them, producing a train of evils connected with it also. With so many causes of mischief co-operating to destroy life, we need not wonder that its span is shortened in all of them by many years. I entertain no doubt of the practicability of correcting the whole mischief; and it is a subject calling for legislative interference, for of the parties concerned, the lamentable recklessness of the one, and heartlessness of the other, stand in the way of private efforts.

self-destroyer, by this demonstration. All it proves is, that there is yet some supplementary air in the lungs, which, breathed out at the moment of the demonstration, leaves quite enough room for a *respiration* of full amount to be carried on for the time, and even for the stays all the while to be made to appear loose about the chest.

A main cause of mischief in such cases is, no doubt, the gradual obstruction of many of the cells and smaller tubes, and the obstructed circulation of blood which it produces in the lungs. But an important part of the mischief, there can be little doubt, is the letting in fresh air too deeply into the lungs. Hence, we shall always find that persons whose vanity, or whose occupation, has reduced greatly the habitual stock of resident air, are those who cannot draw in air freely without a tendency to cough; whereas they who have grown "full-chested" are commonly able to imbibe air suddenly and fully, even when the irritation of a low temperature is added to that of its richness in oxygen. In fact, this richness is suited to the membrane of the upper parts of the lungs which it visits, but such pure air is irritating to the deeper and more delicate vessels, to which it pierces in the other individual. A fact, mentioned by Mr. Thackrah, that he found men able to breathe out nearly double the volume of air which females of the same age could — the former, on an average expiring six pints of air, and the latter, only three and a half — is in exact accordance with the positions here laid down, while important conclusions may be drawn from such a fact, when thus explained. Though I think he has considerably underrated the quantity of air in both, and especially in females, I find the same general truth, in the case of the sedentary females whose chests I have examined, that their resident air is out of all proportion too small, especially in the pulmonic.

These views are not interesting only as matters of theoretical speculation, they are of considerable practical importance. We have seen that the region of respiration differs greatly in different individuals, some breathing upon much more resident air than others; and it is plain that the causes which produce this difference in different persons may operate upon the same person at different periods of his life. To what extent the resident air may vary in the same person at different times, we have not any sufficient data upon which to rest an opinion. It is probable, however, that it may amount to 60, 80, or even, perhaps, 100 cubic inches more at one period than at another; that is, the same person, in an extreme case, may, by habit, have a difference established equal to this in the quantity of resident air in his lungs at different periods.

We may now understand how it is that blowing wind instruments is sometimes beneficial, and how it is generally injurious; also, in what the benefit to pulmonary invalids of practising full respiration really consists. A person, who in blowing a wind instrument does not fill his chest first, but continues the blast until he has driven out as much of the resident air as he can expire, and then makes a deep inspiration, is very likely to bring on disease in the lungs. By such an action he repeatedly subjects the more delicate tubes to the presence and too stimulating action of pure atmospheric air, such as ought not commonly to find its way in so far. But he who by practice has learned to keep a large stock of air in the chest, and who first breathes in, may then blow an instrument with vigour, and with benefit rather than injury.

In the case of the pulmonary invalid whose respiration we would improve, whether we shall do him good or harm, will depend much upon the kind of advice we give him. It is not enough, nor indeed safe, to tell him to practise drawing air in deeply. If he does so upon a chest constantly stinted of its resident air, he is likely to do himself injury by the air he thus introduces more deeply into the organ than nature ever intended. If we will begin by interfering with his breathing, we must tell him, having taken in breath, to endeavour to retain some portion of it when he breathes out, always trying to retain some of the air by not breathing out quite so far. We may thus aid other measures for establishing a fuller and broader condition of the chest. The exertion of blowing through a tube, which has been by some recommended, it may be seen, acts in this manner, by urging a person to lock up as it were a volume of air in his chest so frequently, that at last a fuller habit of the lungs is brought about; that is, a permanently larger quantity of resident air, which, by keeping the cells of the lungs more full, favours the circulation of blood in the vessels accompanying the cells, and serves as a bed of air preoccupying the cells and smaller tubes, and preventing the fresh air of the breath from descending too far before it is well diluted. Hence air may thenceforth be more freely drawn into the chest without causing irritation.

From this we may clearly perceive how blowing wind instruments may either be very injurious or otherwise, and how careful the physician should be in the kind of directions he gives to pulmonary invalids, respecting the practising of a *deeper* respiration. It would be well to employ the term *fuller* rather than *deeper*, there being an essential and serious difference between the two.

A deeper respiration, unless the resident air below it be first increased, does mischief. That it would, we might argue from the provisions of nature. It is natural (for it is the state which prevails in the chests of the vigorous) that the quantity of the resident air should be very large when compared with the air of respiration. Whenever the latter is increased, the body of resident air which is to receive it should be increased also. It being unnatural (for every healthful provision of nature is opposed to it) that the cells should ever be subjected to the presence of *atmospheric* air, which air is greatly more oxygeniferous than that which the Creator has determined shall ever occupy the cells; such being the case, it is of importance, that the directions we give to the unhealthy breather should not be the mere empirical result of common-place observation—such as the maxim, that it is healthful to breathe deeply—but our directions should be founded on a careful study of pulmonary pneumatics—a philosophical acquaintance with the statics of the human chest. We shall then guard ourselves against giving advice actually opposed to the sanative arrangements of nature; which have provided that when, by bad habit, the resident air shall fall off greatly in quantity, the ventilating (*i. e.* respired) air shall decrease also. We must teach ourselves, that the mischief commenced in the decline in quantity of the resident air; and that, so long as this quantity is very limited, we must view a lessened volume of the ventilating air as a temporary arrangement to guard the deeper parts of the lungs from the presence of air too near to the outer air in all its qualities. We shall then be rather careful in telling a pulmonary invalid to force himself to inspire deeply; we shall rather have recourse to some means which we have discerned to favour a growth of the fulness of the chest; that is, a durable increase of resident air, *pari passu*, with a fuller respiration. Not desiring the *latter* without the *former*, we shall be rather unwilling to see an invalid play tricks with his respiration; we shall prefer setting him about measures which tend with certainty to expand the chest—such as throwing back the arms, walking and riding exercise, and, by degrees, the use of dumb bells. As more and more air is rendered resident, we are sure to see the respiration become freer. Let us only gain the former point, and the latter, we may believe, will take care of itself.

On the other hand, if we direct the narrow-chested invalid to *commence* by breathing in and out deeply, before habit has increased his stock of resident air, we subject the more delicate portions of his pulmonary membrane to the presence of air of a quality which nature has not intended should ever visit them, even in their healthy state; and against which intrusion, when the resident air had declined in quantity, we ought to perceive the feebler respiration to be an especial provision.

It may and does happen sometimes that the deepened respiration is endured without injury, until an increase in the volume of the resident air is established; but it must be because this increase, being natural, takes place soon. This subordinate fact does not at all affect the principal one, that a respiration naturally deep, upon a chest less than naturally full, is an unnatural arrangement. Being *unnatural*, we might assure ourselves it was improper; and we have seen other and abundant evidence that it is so.

In the natural division of the *resident* air of the chest into the two portions, which have been considered under the names *residual* and *supplementary*, we may discern a wise provision for important ends. It was important that *all* the *resident* air should not be in the predicament of its *supplementary* portion, that is liable at will to be entirely expelled. In that case, the very cells of the lungs would at times have been exposed, upon the indraught following the extreme expulsion, to pure atmospheric air, against which, it has been shown, the general presence of a body of resident air is an especial provision. This provision may assure us that it would be injurious for an entire expulsion of all air to take place, even temporarily. Hence we cannot expel much more than half of the resident air; or in other words, hence the provision of *residual* air, which cannot ever be expelled.

The total expulsion, also, would have obstructed the circulation of blood through the vessels of the lungs too seriously to be endured; and it would have deprived the blood of all interchange of gases for the time, which, however transient, would have been, we may believe, insufferable. These several reasons are, doubtless, enough to account for the incompressibility of the chest, after the resident air is reduced to about 100 or 120 cubic inches, which quantity remains in as a residual stock, enough to guard against the extreme effects noticed above.

Again, while a portion of the *resident* air is thus absolutely retained as a *residual* stock, we may find a ready reason why the *whole of the resident air* was not so posited; why, since this larger stock is permanently required, the incompressibility of the chest was not fixed at a point of inflation which would have included and detained the whole of it; thus making this *resident* air no longer divisible into a supplementary and residual portion, but merging the former into the latter, as one large *residual* quantity. The *resident* air would then be all of it *residual* air—air which could not be expelled; and the two terms would be synonymous, instead of the one including the other as a smaller part of it. Had this been the case, the action of coughing could not have been as freely performed, as the expulsion of phlegm and foreign substances from the chest or windpipe often requires; for the air which is chiefly employed to drive them out, is the supplementary or expulsible portion of the resident air.

Lastly, the collateral, but very important duty of the chest in speaking, especially in oratory, requires the command of both the supplementary and complementary spaces. The duration of an act of expiration is greatly increased in giving expression to a long sentence. The chest has to be nearly filled with air: the air, occupying almost the whole of the complementary space, is first spoken forth, then that of the region of the breath; and in a long sentence, forcibly uttered, a large demand is also made upon the supplementary air. But for this long range, there could be no powerful *eloquence.* At the same time, a loud voice and long sentences make so frequent and large demands on the supplementary stock, as to subject delicate portions of the pulmonary membrane to the frequent presence of undiluted air, against which the supplementary air was especially provided as their natural protection. Hence these efforts either by degrees inure such delicate parts as are visited by the inhaled air to its action; or, as too frequently happens, the air gains the better of them; irritation is excited; and, if the efforts are persevered in, disease is established. By employing very short sentences, and by habituating the chest to receive a full *complementary* quantity of air, that quantity, together with the ordinary region of breath, will be found to suffice; so that the resident air need not ever be intruded upon. It is of great importance in such cases, that this *resident stock* should be also of full quantity; occupying steadily its protective position; there receiving all the impulses of quickly-inhaled breath; duly modifying the portion of it retained; and gradually incorporating it into itself as resident air, before conveying it down into the cells. It is probable, that many a preacher might continue in his vocation, by carefully attending to this simple rule. Indeed many, no doubt, practise it instinctively as a matter of experience, without inquiring into the physiological reason.

I have already noticed, amongst other uses of the resident air, the important purpose it must serve in lying as a barrier between air inhaled of an irritating quality, and the more delicate surfaces of the lungs. Thus dust, acrimonious particles, extremes of dryness, of cold, and of heat, are deprived of any sudden effects upon the deepest portions of the lungs. Allied to this, I cannot omit to notice a duty of great importance, which I have long believed the resident air to perform; namely, that of diminishing the influence of miasmata and other infections.

With considerable opportunities I have endeavoured to form a right judgment as to the manner in which the system falls under the influence of marsh, jungle, and other poisons conveyed through the atmosphere. Without at all questioning the possibility of the action taking place through the skin, all the evidence I have been able to collect from observation has led me to believe that, in a large majority of cases, the lungs are the channel through which the poison acts.

When the epidermis is soaked by a watery perspiration, or by wet being applied outwardly, it has appeared permeable by poison, which such fluid may receive in solution and convey inwardly, and I have known fevers and dysenteric attacks traceable with some reason to such action. But when the epidermis is ordinarily dry, it is, I conceive, very questionable, if these poisons often produce their action through it, either by absorption or by affecting the nervous system.

Considering the lungs as the usual channel through which atmospheric poisons exercise their influence, we at once perceive the high importance, in such atmospheres, of the large body of resident air. Each breath, on entering, suffers repeated dilution before any of it can reach the cells of the lungs, and must have its morbific virulence lowered in proportion. It cannot be unreasonable to consider that the susceptibility of a person to its influence will, in no small measure, depend upon his stock of resident air being large, in proportion to the bulk of each breath. Under this impression, I have, in my own case, been always careful in India, when exposed unusually to the influence of malaria, especially when travelling at night through a jungly country in the rainy season, to endeavour to keep the chest pretty full of air, and to draw upon that stock shortened inspirations; their small quantity being made up by frequency, and all deep inspirations being avoided. A sequence of this precaution, so invariable as to wear the aspect of a consequence of it, has been an immunity from all disease occasioned *by malaria* throughout a service in the East of many years' duration, and, at times, of more than common responsibility and labour; while persons of superior health and vigour were frequently attacked. In the more trying case even of jungle malaria, it is not improbable that the *resident* air would often temper down poison in the respired air, by a frequent dilution before admitting it to the finer membrane of the lungs, and that it does so in many cases, but that an accidental yawn, or sigh, in the heart of a jungle (an action otherwise congenial with the place), may seal the traveller's fate, by an indraught of poison more than a match for the custodiary power of his resident air. The subject is worthy the attention of Indian travellers.

It will not be out of place to invite the reader's attention here, to a point connected with the position of the abdominal viscera of considerable pathological as well as physiological importance, which is dependant upon the presence or absence of the supplementary air of the chest.

When the abdominal cavity is laid open in *post-mortem* examinations, or for anatomical instruction, we find the whole of the liver, excepting at the ensiform cartilage, under the ribs, their margin bounding its margin, and the position of all the other viscera bearing a relation to its position. The greater portion of the stomach is hidden under the lower part of the sternum. It is when they are in this predicament that we study their position. Hence, with respect to the liver, we are taught, in the anatomical school, that in a healthy person its margin is to be found under the ribs, even in the case of the right lobe, and nearly following the course of their lower edges. And we are commonly further taught, that if the organ can be felt stretching down towards the umbilical region, we are to consider enlargement of it to have taken place.

A correct view upon this point is of especial importance in tropical countries, where liver disease prevails so much. I have known the above view to lead to the opinion that the liver was greatly enlarged when it was only in a congested state.

We have to bear in mind that after death the resident air has lost the whole of its supplementary quantity, its residual portion only remaining; and that the space in the chest for the large supplementary quantity was, in life, chiefly provided by the diaphragm being stretched across between the chest and abdomen, in a curve much less arched upwards than we find it after death. It is plain, then, that in life the liver must be proportionally lower; its boundary no longer corresponds with that of the ribs, but is much lower down, *being entirely determined by the quantity of the supplementary air in the chest above.*

What this quantity is, and how great the change in the position of the viscera must be, we may form some judgment by the difference in the outward appearance of the belly in life, and after death. In life, a line extended from the sternum to the pubis will in general touch the navel in its way. In the upright position the abdomen is rather convex, and would push such a line into a curve forwards. How great is the contrast after death! The surface of the belly recedes far within such a line, becoming, on an average of cases, more concave than it was convex before. This concavity gives to a corpse its most characteristic appearance. This great difference of space was in life in the chest, and occupied by the supplementary air. Then the liver and stomach were, as it were, below this space, and helped to fill out the belly into a cylinder symmetrical with the chest; now, however, they are, as it were, above the space. It has left the chest with the supplementary air; the diaphragm has been pressed far up into the region of the chest by the abdominal viscera; the liver and stomach are therefore so much higher than they ought to be, and the space which formed part of the chest is now manifested, below the liver, outside of the belly, in its very hollow condition.

In life, then, the liver is only partly covered by the ribs. It descends much below them; but it is not readily felt in health, on account of its softness. In a highly congested state, however, it becomes hard, and suddenly appears where it seemed not to have been, where we had not been taught to expect it. This is a point worthy of much attention. For want of a due consideration of this change of position of the abdominal viscera after death, by the efflux of the supplementary air, I believe extensive enlargement of the liver has often been supposed to exist in cases where it was only in a congested state, and therefore harder and more readily discernible.

For this reason, and for a right anatomical view of the abdominal viscera, it would be well if the cavity were not laid open before the student, until the lungs had had their quantity of supplementary air restored to them by inflation through the trachea, and its residence secured by a ligature round the tube. The abdomen would then be brought into its natural state, the belly being filled out with the descended viscera.

In concluding this division of my subject, a remark may be offered on the figurative, but general use of the word EXPIRE. When the physiologist speaks of a person *expiring* many times in a minute, though he employs correctly a word which means breathing out, he has often to explain himself, even to persons as well acquainted with the Latin as himself; so generally is *death* associated with the verb *to expire.*

In the horizontal position, the abdominal viscera press sideways against the diaphragm. In the last moments of life this muscle becomes too feeble to re-act against them. We may perceive each act of expiration to grow longer than the succeeding inspiration ; a portion of the supplementary stock of air is being added to each outgoing breath, until the diaphragm, being deprived of all vital stimulus, becomes entirely relaxed ; it is then pressed far up towards the chest, into a deep concavity by the gravitating viscera, and all the supplementary air is expelled, in one long last act of expiration ; the person has then emphatically *expired.* Hence, by a metonomy, to expire, is to die. The long and solemn sound from the vibrations of the relaxed vocal organs produced by this lengthened expiration, has at all times caused this final act to be associated with death as its constant accompaniment. It does, however, neither necessarily, nor always take place.

A person dying with the body in an upright position will not *expire* as here described ; his final expirations will not carry out the bulk of the supplementary air. The viscera do not press up towards the chest, but rather gravitate downwards and forwards. The diaphragm will retain more nearly the position it had in life, and most of the supplementary air will remain in the chest. Death may thus take place without the usually accompanying sound, which has given to *expire* an emphatic and figurative meaning. Hence we find persons sometimes to die in a chair, unperceived by others present : they have not made the final and audible expulsion from the chest of supplementary air, which usually gives notice of the event. Even in the horizontal position, in cases when death takes place very gradually, a feeble person may lose all the supplementary air by degrees, so that, at the last, none remains to produce the final solemn sound. Thus aged persons often die at a moment unobserved.

PART II.

VIEWS UPON ANIMAL HEAT.

CHAPTER I.

The measures we adopt for the preservation of health, and the cure of disease, must, in numerous instances, be much influenced by the views we entertain respecting the production and dissipation of animal heat. It is therefore of great practical importance that our theory of animal heat should be a subject of frequent examination and inquiry, that it may steadily progress towards a faithful representation of the operations of nature in this curious work.

It has pleased the Creator, for reasons we assuredly cannot discern, to constitute the corporeal fabric, in the higher class of animals especially, of such a nature as to require to be maintained at one determined degree of heat, and to suffer a loss of health, and even of life, whensoever the body shall vary more than a few degrees from the point of heat ordained for it. At the same time no immunity, so far as we can discern, has been granted us from those laws of nature, which determine the movements of heat. Hence, without some especial provision, our bodies would, like a statue, be constantly varying in temperature, growing hot or cold with every change in the air around us. A degree of heat which we find so fixed as to vary scarcely more than from 98° to

100° of Fahrenheit's scale being that determined for us in a world, the habitable portions of which have a mean temperature 40° or 50° lower, can only be maintained by some powerful means of producing heat within us.

Greatly as we are indebted, first to Crawford for his masterly researches, and for his theory of animal heat founded on them, and since, to numerous able inquirers, much remains yet to be effected before our knowledge of this important result of vital action can be pronounced by any means complete.

The connexion of animal heat with the changes going on in the fabric of the body, and with the food, and the influence of climate on its production, are subjects which will require long and careful investigation.

I have already noticed* the advantageous circumstances in which an observer is placed in a new climate, and amongst races of men whose habits and diet materially differ from those which afford the data of usual observations.

Under such favourable opportunities for exciting inquiry, the following points have long appeared to me to merit serious consideration, on account of their high interest and of their importance, and to invite our adoption of certain views, as the necessary results of correct reasoning upon them.

1. Are the sources of animal heat similar to those which yield the heat in combustion, and what relation does the production of animal heat bear to the *temperament* of the atmosphere in which it is placed?

* Preface.

2. Are the elements which pass off from the lungs derived from the waste and absorbed fabrics of the body, or directly from the food, or from both these sources; and is the whole food, with the exception of the excrementitious part, convertible into the corporeal fabric, or only such portion of the food as corresponds with the bodily fabric in its elementary composition?

3. Is the nitrogen of the atmosphere of no other than passive service as a diluent of the oxygen, or is it actively engaged in the transformations which take place in the body?

4. In what parts of the system does the development of heat take place, and are the capillary vessels actively engaged in the work, and in the propulsion of the fluid circulating in them, or merely passive tubes?

Under these four heads are comprised questions of very high importance, and as interesting as they are important. We must not enter upon our task without, as a preliminary consideration, reverting to the fact, earnestly set forth in former pages, that it is not the air of respiration which does, or can, act directly on the blood, but the resident air of the chest; therefore, that we must suppose atmospheric air in its pure state to be unfitted for the work (otherwise the Creator would have given it access to the cells), and that the air of expiration is not that previously inspired, but is chiefly derived from the resident air, most of the inspired air having at the same time become resident. Hence, when we compare the expired with the inspired air, we must bear in mind that their difference is not a change wrought upon the latter by its commerce with the blood, for it never falls within reach of it, but an average indication of the business going on, which keeps the resident air so far deoxydated and carbonated, that its upper portions throw off the expired air, of the quality we find it.

CHAP. II.

We have then to inquire, first, Are the sources of animal heat similar to those yielding the heat in combustion, and what relation does the production of animal heat bear to the *temperament* of the atmosphere in which the body is placed? With respect to the latter question, I am not aware that it has yet received attention. Much has been said on the effect of *temperature*, but the combined influence of heat and moisture, which I take the liberty of comprising under the word *temperament*, will be found, I conceive, to require our admission of a fact, involving, as its necessary consequence, a considerable modification of the theory of animal heat at present adopted.

First, then, as to the source of animal heat.

In all ordinary cases of combustion, as of fires or lamps, we find, of the matters yielding the heat, the *visible* portion to contain two elements chiefly, the union of which with oxygen presents the familiar and useful instances of the development of terrestrial heat. In charcoal we have one of these, carbon, acting alone; while in bituminous fuel, as coal, and especially in oils, its heating agency is assisted by hydrogen, which enters largely into their composition.

It was a happy conjecture of Crawford, and ably supported by his experiments, that the union of carbon with oxygen was a chief source of animal heat, and of Lavoisier, that this union was aided by an union of hydrogen with oxygen; the heat developed having a double source, hydrogenous as well as carbonaceous, as in the case of the combustion of oily bodies. This view was favoured by the observation made, first by Priestley, and subsequently by Lavoisier and others, that more oxygen was lost in respiration than was represented by the carbonic acid exhaled from the blood. A doubt having been raised as to this fact, most physiologists have relinquished the opinion that an union of hydrogen, as well as of carbon, with oxygen, forms part of the process. Yet it will appear that no such extra loss of inspired oxygen is necessary for establishing the fact that water is in truth formed in the system.

In Bengal at certain times, by a condition of the atmosphere about to be noticed, our animal heat appears almost wholly shut in. I was led by this striking fact to contrast with it the great outgoing of heat in arctic regions; and having heard that the latter climate induces a demand, not only for more food, but especially for such as was of an oleaginous description, the truth of Lavoisier's position was strongly impressed on my mind. It appeared, at the least, highly probable that there was an immediate connexion between the nature of the food and the development of heat; but a further attention to the subject of diet rendered it obvious, that, not only in the case of oleaginous diet, but in that of every variety of human food, hydrogen, as well as carbon, must find vent from the system by combining with oxygen, and thus be available as a source of heat.* The numerous experiments of accurate

* Appendix II.

observers afford us sufficient information as to the exact quantity of all the egesta, or total discharges from the body, as the basis upon which to reason.

To proceed with logical precision, we have to keep in our view a fact, obvious enough when our minds are directed to it, but one which has been too much lost sight of, that whatever substances pass out from the body, whether solid from the alvine, liquid from the renal, or gaseous from the cutaneous and pulmonary outlets, must all have been derived, directly or indirectly, from matter taken into the body; and that when the body is in a stationary condition as to the quantity and quality of its materials, as in the case of an adult who is not varying in weight, all that is given out must collectively amount exactly to all that is taken in, *corresponding with it precisely both in quantity and in elementary composition.*

Of the four outlets of the body we find two, the skin and the lungs, to perform the duty of inlets. All the gaseous matter absorbed then by them, when added to all that enters the stomach, must, when the body is not varying in bulk nor in the quality of its materials, be exactly represented, as to its *elementary* composition, by all the discharges by the four outlets taken together. Whether the ingesta shall, in whole or in part, have gone the circuit of forming corporeal substances, and of then being absorbed into the blood again, or whether (if that could happen) all of it should be directly discharged from the various outlets wholly unemployed in nutrition, it must be obvious upon reflection that in either case the elements which leave the body, however changed in their combinations, must be identically the same with those which enter it, or exchanged for others of the very same nature and quantity; for if this were not the case, the body itself must have varied in the quantity or quality of its materials. Therefore, whatever elements are taken into the stomach as food, must be accounted for in the discharges.

Obvious as these truths appear when once stated, had this simple process of reasoning been employed, it would have aided much the advancement of the theory of animal heat. Thus it would have placed Lavoisier's views respecting the union of hydrogen with oxygen in the system beyond a question; it would have advanced them from being a matter of supposition merely, to one of demonstration. When we hear of the oily nature of the food selected by dwellers in "arctic" regions, we may, knowing the hydrogenous nature of that food, and that hydrogen may yield heat largely by uniting with oxygen, *conjecture* that such an union does take place, and that it aids that of carbon with oxygen in developing the large quantity of animal heat needed in those latitudes. But, put in this form, the view, however inviting and probable, has only the character of a conjecture. When, however, we proceed with the above fact before our sight, namely, the exact correspondence between all elements entering and going out from the body, we may prove the conjecture of Lavoisier, that water is formed in the system, to be an unquestionable truth.

Thus, the inquiries of Lavoisier himself, of Rye, Dalton, Berzelius, and others, prove, when compared together, that the quantity of carbon discharged in the form of carbonic acid from the lungs alone, exceeds very greatly that contained in the alvine discharge, that it exceeds even the whole average weight of the fæces, of which carbon forms the smaller portion only. Now, as already remarked, the carbon excreted from the lungs and skin must be all derived from the food, whether directly it is digested and introduced into the blood as chyle, or indirectly after it has gone the circuit of forming part of the body, and returning as waste matter to the blood. Again, we have to consider that this carbon, when it entered the stomach as food, was not and could not have been introduced alone, *charcoal* not forming an article of diet; but that, whatever was the food in which the carbon entered, whether of animal or vegetable origin, it was in that food associated with a large quantity of hydrogen. Now, allowing the pulmonary discharge alone of carbon not to exceed eight ounces a day (by many it is supposed to equal twelve ounces), we have the hydrogen which associated it thus stripped of its carbon, and left either to unite with the remaining carbon and other elements of the food, or with nitrogen alone, to form ammonia. Now we know that the fæcal discharge contains no such quantity of hydrogen, indeed that there are no solid compounds of hydrogen ever in it which could carry off so much. Again in the urine, the urea and ammonia, the chief compounds of hydrogen, do not convey away from the system the fourth part of the quantity of this element, which is liberated from so much carbon as is daily discharged in the form of carbonic acid from the lungs, not to mention whatever carbonic acid passes off by the skin.

All the carbon, then, discharged as carbonic acid daily, has been previously combined with a quantity of hydrogen in the food much larger than could find vent in any other hydrogenous compound, except water. If, then, water were not formed, there would be daily a large quantity of this element evolved alone, and therefore in the bulky and distressing form of hydrogen gas.

Again, if we look for the other element of water, oxygen, we find a redundance of it also to be disposed of. In nearly all kinds of food the oxygen present is enough, when the carbon is removed, to form water with the hydrogen also present. Now, none of this oxygen is required for converting that carbon into carbonic acid, for respiration supplies enough, or apparently more than enough, oxygen for that purpose. It has been seen that the fæcal and urinary discharges can carry off but a small part of the redundant hydrogen, and it might be shown that they afford an outlet for but a small part of the superfluous oxygen liberated from so large a quantity of carbon. It may be well to urge again the recollection of the truth that we need not complicate the inquiry by following the nutriment into its assimilation into flesh, for whatever elements enter the body within a given time, similar in nature and quantity, must in the same time pass out of it.

We have, then, before us both hydrogen and oxygen largely set free by the withdrawal of carbon daily in the form of carbonic acid. We know that they do not find vent by any of the outlets in their separate and gaseous form, but we do find that three of the outlets carry off a large quantity of water daily. Can we then doubt that a portion of this water is formed by the union of that oxygen and hydrogen, the exit of which are both known to take place, and also to take place in no other form?* The conclusion is irresistible. When decided, the fact forms an inseparable and very important part of the theory of animal heat, and its rejection by the majority of recent authorities renders such a discussion and settlement of it a step necessary to the progress of our knowledge.

We must then relieve our minds of all doubt as to the correctness of Lavoisier's opinion, that a formation of water is continually going on in our bodies by a union of its elements, since an inseparable condition of the abstraction of so large a quantity of carbon, as that exhaled in the form of carbonic acid, is a liberation of oxygen and hydrogen in such quantity, that no vent for it can be discovered but by their uniting to form a part of the water which issues abundantly forth by no less than three out of the four natural outlets.

If, then, the sources of animal heat are the same as those yielding the heat in combustion, the second

* I once found a mouse which had been confined for a long period of time in a tub of very dry flour, and that in a very dry atmosphere. It might be supposed that, even from such an atmosphere, nature, in its extremity, absorbed more moisture by the skin and lungs than was exhaled by them, and thus supplied the juices of the body, and the urine necessary for keeping up the function of the kidneys, and carrying off the nitrogenous excretions and urinary salts; but it appeared to me more probable that water formed by the union of the liberated hydrogen and oxygen of the food (flour) was retained in the system to supply the juices; and that it then passed off partly as urine, and in part, perhaps, by the lungs and skin.

portion of our question presents itself—what relation does the production of animal heat bear to the temperament of the atmosphere in which the body is placed?

The fact did not escape the discernment of Crawford, that more oxygen was absorbed, and more carbonic acid thrown off, by the lungs in cold than in hot weather. Confirmed as the same fact has been by the experiments of Jurine and Lavoisier, and more recently by those of Dr. Edwards, it points to the reasonable conclusion, that the quantity of animal heat generated bears some inverse proportion to the temperature of the air. It has been been rightly observed by this able physiologist, and by others* also, that the lesser density of warm air would cause a given volume entering the chest to convey in less oxygen than in the case of cold air: but I do not think much stress ought to be laid upon this point. One hundred degrees of temperature constitute nearly the extreme range of habitable climates, while 60° or 70° may be considered a full annual range of most. Now the former would only affect the density of the air by about one fifth.

From what has been said under a former head, we can have little doubt that the bulk of our inspirations might readily vary that quantity, if requisite. As, however, in a high temperature we need less heat, we may certainly conclude that our respirations do not increase in volume, and therefore that the diminished density of the air may so far favour a diminished production of heat. But, as will presently appear,

* Appendix I.

a much greater diminution than could result from this cause alone has to be admitted, before we can at all comprehend the endurance, for a day even, of such atmospheric conditions as at times prevail in the tropics.

Again, if a diminution of the density of the air involved with it, as a necessary consequence, a diminished production of animal heat, mountain heights of even moderate loftiness would be uninhabitable. In the intensely cold and clear atmosphere of lofty mountains very much more heat is needed than at the surface of the earth, and it cannot be generated without much more air. To make up for there being less oxygen in *a given bulk*, the bulk of the air respired must be much larger. This, if there be any truth in the theory of animal heat, we might *à priori* affirm as a necessary consequence of the theory: but we may bring experience to establish it. The majority of travellers to great heights have confirmed the observations of Saussure. The symptoms they generally describe are such as indicate a need at great elevations of a freer respiration than they had been accustomed to.

My own opportunities have been particularly favourable for studying the effect of lofty ascents on the respiration, and have established in my own mind the conviction that the volume of the air respired increases with the height, not perhaps in the inverse ratio of the density of the air, but yet very considerably. Though in the latitude of the northern Himalayas, on which mountains the observations were made, I did not *perceive* a much freer respiration than usual to be called for, below a height of

9000 feet above the level of the sea, at that height, and above it, a more voluminous respiration was very perceptible ; and this, not during exertion only, but while at rest also. I have no doubt the chest commenced its extra expansion as soon as ever the rarity of the air made any material difference in the quantity of vital air contained in a usual volume of breath, though the increased expansion was not perceived until augmented by so considerable a height.

At a height of between 11,000 and 12,000 feet, though a feeling of exhilaration was felt, and the respiration was performed with great freedom, there was every now and then a disposition to draw a deep breath or sigh. Though keen and frosty, the air was no more cold at that period of the year (the middle of September) than I felt to be invigorating. It was no effect from cold acting on the nervous system, nor had it the transient character of a shock like the hurried respiration caused by a cold bath ; it was a steady enduring effect of the rarity of the air, observed during several successive days spent between the elevations of 10,000 and 14,000 feet.

At a still greater elevation my own respiration was embarrassed by an amount of exertion which would not have affected it in the least upon the plain surface of the earth ; and I observed the same in such natives as I had brought up from the plains with me. While attaining a height between 16,000 and 17,000 feet, the utmost to which I ascended, I experienced, in a manner not to be overlooked nor mistaken, an increasing necessity for fuller respiration, together with the peculiar and sudden prostration of strength described by Saussure. The mountaineers with me came from a village 10,000 feet high, and I perceived that they experienced little of the feeling, arising no doubt from their habituation to fuller respiration: they were well aware of the effect on strangers, and prepared me to expect it.

With respect to the sudden prostration of strength, I believe it may be shown to arise, in addition to the state of the respiration, from an insufficient pressure on the brain; a subject, however, foreign to that before us.

The greater density of the air in winter, and therefore greater proportion of oxygen in a given bulk, which corresponds with the greater demand for oxygen at that cold season than in summer, we cannot then consider as the peculiar and only provision for giving a larger supply at the time it is needed; for, in the first place, we have seen that the intense cold which prevails in winter at the mountain village, for instance, to which I have alluded, must call for a supply of heat as great as in the very northern parts of Europe, while instead of an increased density of the atmosphere, the opposite condition of great rarity obtains. This case, as we have already seen, proves that the great provision against a varying supply must be found in a corresponding variation in the capacity of the chest, and this variation in the capacity of the chest (established in such cases beyond all question), also offers to our view at once the real and abundant means by which an increased demand of oxygen *on account of cold* is supplied. Thus, in the mountain case before us, the increased capacity of the breath must be viewed as twofold; first, to make up the difference of the oxygenous supply arising from diminished density; and, secondly, to superadd the extra quantity of oxygen required on account of the intense cold at that elevation in winter.

In the next place, the mere increase of the density of the air from cold on the surface of the earth, would not by any means account for the extra quantity of oxygen which must then be consumed to generate the requisite degree of heat; or, in other words, the want of oxygen occasioned by cold, varies in a much higher ratio than the density of the air arising from cold. Thus, in those northern regions where a quantity of oxygen-consuming food is devoured much greater than we can venture to take in our climate, and where the supply of oxygen must of consequence be proportionally greater, if we allow as much as fifty degrees as the average difference of temperature between the winter there and here (an extreme allowance), we shall find that it would only add one tenth to the quantity of oxygen in each equal volume of inspired air; *one tenth part*, where *four times* the quantity is required to correspond with all the carbon and hydrogen in the stone weight of fat which the Laplander will eat at a meal.

There can be no question, then, that the explanation afforded by some of the larger supply of oxygen when the temperature is low, namely, that then the air is more dense, is altogether inadequate to account for the supply needed. It is a happy coincidence as far as it goes, which only holds good indeed under the same barometric pressure; we must then look for an explanation of the increased supply of oxygen elsewhere, and we shall find the main source of an extra supply in the fact, which from its importance cannot be too strongly impressed on our minds, that the volume of the air of respiration is ever variable; the breathing being increased when more oxygen is wanted, and decreased when less. In one case it must be, and is increased, as at great elevations, to supply the deficiency arising from rarity. It is still further increased in winter, at the same elevations, to supply the extra demand from cold. In another case, on the plain surface of the earth, the bulk of the respired air has to be increased at different seasons, on account of the demand of oxygen from cold alone; and in a third and opposite case to these, the bulk is unquestionably *diminished*, — namely, during periods of atrophy or inanition, — to husband the materials of the body as far as possible; a fact perceptible to view in such cases, and established beyond doubt from other less extreme cases, where very small quantities of food are consumed during a period of many years, yet emaciation does not proceed beyond a certain point, but the body remains in a stationary state as to weight.

We may now perceive another reason, in addition to those noticed in the first part of this work, for that peculiar structure of the chest which places the air of respiration intermediately between the supplementary and complementary space; so that after a full ordinary inspiration, if need require it, an additional quantity of air may be drawn in, corresponding with a portion of the complemental space. And again, if needful, the expirations may be increased, breathing out being carried to a greater extent.

In this case the respiration will intrude upon the usual supplemental space. In this manner by habit an increased respiration may be, and there can be no doubt oftentimes is, permanently established.

So likewise in cases of an opposite nature, when, upon the whole, a diminution in the production of heat is a smaller evil than an union of the elements of the blood with oxygen greater than the supply of food at the time admits of, the respiration may, and evidently does, become *lessened* in volume, to accommodate the oxygen introduced to the smaller supply of food. In this case a portion of the space usually occupied by the air of respiration will be added to either the complemental or supplemental spaces, according to the character of the individual's respiration, — a point already explained.

The point under consideration is not one of scientific interest only, it directs us to objects of practical utility; for the fact of inanition from feeble digestion leading of necessity to a correspondingly feeble development of heat, led me to view the recovery of animal heat lost by the lungs as an object to be desired, not only for giving *local* protection to the pulmonary invalid, but also for saving the warmth of the system generally, which the dyspeptic, who dare not eat an adequate quantity of food, could ill afford to part with. If the heat passing off by the breath could be caught in its way out, and restored to the lungs, it appeared evident that all the more would remain in the blood, and, circulating with it, would warm the extremities. This expectation held out an additional and interesting encouragement to prosecute the experiments which terminated in the respirator ; and I ventured to express a confident anticipation of this effect of the instrument in papers descriptive of it at the time of its production.

Within a year afterwards the case was related to me of a medical gentleman, who stated that he did not require the respirator for the protection of his lungs, but that he found it a warmer of his limbs, which were otherwise always cold, on account of his delicate health from *extreme and habitual indigestion.** Such a case sets forth in a striking manner the fact that the consumption of oxygen in the lungs must diminish with a diminution of the food. Though insufficient animal heat is its consequence, it is not so great an evil as would be the development of more heat, at the expense of a quantity of carbon larger than the system could spare when so ill supplied with food.

I have said, in the case of a varying demand for oxygen, whether the supply has to be increased for the generation of more heat, as with persons exposed to cold and able to eat heartily, or whether it has to be diminished, as in a dyspeptic, who cannot supply the usual quantity of carbon and hydrogen, that we must view *one* chief provision for varying the demand to be the peculiar structure of the chest, by which it may readily be habituated to a respiration either more or less voluminous than usual. It has appeared a proper caution to name this as *one*, and not as

* Many instances have since come to my knowledge of this indirect use of the respirator, especially in persons suffering from coldness of the limbs in bed. In some of these instances, after every other means had failed to warm them permanently, it proved effectual.

the only providential arrangement for the purpose. Another, the existence of which is highly probable, offers itself to our view.

We have seen that the *air of respiration* takes no direct part in the oxydation of the blood; that the direct duty is wholly performed by the voluminous and deeper seated *resident air*. Now, it must be obvious that according to the quickness with which the former air is impelled upon the latter, must be the completeness with which it—the air of respiration—mixes with the upper portion of this, the resident air. So, likewise, the force of the next inspiratory impulse, which conveys this mixed portion onwards towards the cells, will determine how much recent air shall enter into the mixed air supplying the cells. In short, the ventilation of the cells depends not only on the volume of the breath drawn in upon the resident air, but much also on the quickness of the inspiration. We might conceive it possible to draw the air of respiration into the chest so gently as not to disturb the resident air at all,—as not to mix even with any portion of it. If it were then breathed out with equal gentleness, it would return, having performed no duty in ventilating the chest; it would, in fact, come back as it went in, having left none of itself behind, nor received in exchange any of the resident air fraught with carbonic acid. Now, although so extreme a case as this may not be attainable in practice, owing to the mobile and miscible character of air, it serves to illustrate the fact of the degree of the commixture, and consequent ventilation of the cells, very much depending on the force as well as volume of the inspiration. Hence, in those very cases of inanition in which the formation of but little carbonic acid can be endured, we may observe the respiration to be *feeble,* as well as small in volume. Thus the ventilation is moderated in two ways; the one by diminishing the volume of the breath, the other by causing, at the same time, a more gradual mixture of it than usual with the resident air. These are facts the truth of which cannot be set aside. We must not, therefore, overlook their importance.

Thus we may perceive a double provision for regulating the ventilation of the air-cells, or movement onwards towards them of fresh air.

At the same time, it can hardly be doubted that there is yet another power which, after the air is absorbed into the blood, regulates the formation of carbonic acid and water. We have evidence on all hands of the strongest affinities yielding to the vital power. The very existence of the animal structure requires this. The unquestionably lower vital action of plants* is yet powerful enough to overrule one of the strongest physical affinities—that between carbon and oxygen; thus we know nearly the whole vegetable kingdom is nourished by carbon, derived from carbonic acid absorbed by the leaf from the atmosphere. If the vital power of the vegetable is able to tear apart these elements when united, can we doubt that the higher vital action of the animal is able to regulate the extent to which they shall unite, — that it does, in fact, determine exactly what quantity of the oxygen brought into the system from the lungs shall be employed, so as to resolve into carbonic

* Appendix VIII.

acid as much carbon as can be spared, and no more?

We have, then, in the first place, a kind of outside arrangement in the lungs, regulating in a general manner the quantity of oxygeniferous air offered to the blood for absorption, and then the internal control of the vital power, determining more exactly the quantity to be employed of that which has been absorbed. The statics of the chest, and all we know of the vital power, urge us to entertain this view, which is both simple and satisfactory.

Of the question under discussion, the relation which the development of heat bears to the *temperament* of the atmosphere in which the body is placed, we have considered above that part which relates to low temperatures. It remains for us to examine the condition of the body as to heat in tropical atmospheres.

We owe to the labours of Delaroche so clear, and, to a certain extent, satisfactory an exposition of the manner in which an excess of animal heat may, and commonly is, carried off when the outer temperature is high,—namely, by the heat becoming latent in the perspiration, which is more copiously exhaled as the temperature rises,—that this provision has, ever since, been viewed as an efficient regulator of the animal temperature. It has been not uncommon even to consider the production of animal heat as invariable, being the same in a hot as in a cold climate, the skin being supposed invested with the power and duty of giving vent to the large excess of heat, which, under such circumstances of an equal development of heat, must otherwise in a hot climate accumulate to the destruction of life.

Here, however, the duty of the skin, important as it is, has been much overrated.

The fact, that there is doubtless an actually smaller development of heat at a high than a low state of the thermometer, has been already fully discussed; and this diminished development of heat, together with an increased exhalation from the skin, might be supposed to offer a sufficient account of the process of relief in a tropical climate.

But how are we to account for the removal of animal heat under the following circumstances? In the rainy season, throughout the vast and populous regions of the monsoons, the temperature of the air commonly ranges between 80° and 100°; and the hygrometer generally indicates few degrees of dryness. In some parts of Bengal I have known, for many days, the heat to average, and at times to exceed, that of the body; while the air was so moist that dry linen grew damp by exposure to it. The whole surface of the country has been under water, and the fall of rain almost incessant. The moistened bulb thermometer indicated evaporation to be at its lowest point. At such times I could not but ask myself, what now is the outlet for the animal heat? Its road by conduction and evaporation being almost entirely closed, if I turned to radiation it was obvious that that avenue was nearly as much occupied by heat projected in from the walls of the house, which were almost as hot as the body, as by heat radiated outwards from the body. Radiation within doors could do nothing. The natives instinctively sought its action, by lying out in the intervals of rain. But malaria, which rendered it dangerous even to them, made such exposure imminently so to an European. And at best the radiation was trifling, under the universal canopy of dense clouds which generally hung in the atmosphere.

This grievous category of atmospheric causes, shutting up almost completely every natural outlet for animal heat, was indeed productive of feelings of suffocation and general oppression, and too often, in the case of Europeans, issued in disease; but how does it fail to prove immediately fatal? It was plain that any thing like the usual amount of heat developed would literally boil a man alive while so sadly well shut in. Yet we do manage to endure even this extreme confinement of animal heat at times; and for many months a confinement of our heat, though less complete, still far too considerable to be endured, if the production of it were not reduced to a very small quantity.

Of the extent to which evaporation has been arrested, the channel commonly supposed the outlet of heat in a tropical climate, an idea may be formed from the fact, that I have known times when brisk fanning of the face produced scarcely any sense of coolness, although the face was bedewed with perspiration. A more striking proof of the confinement of the animal heat could not be afforded than by such a fact; yet the natives are not much distressed, and many Europeans pass through the rainy season without suffering in health.

In endeavouring to ascertain the natural provision for reducing so greatly the production of heat, I was led to contrast with this state of things that of the condition of human beings in the polar regions;

and was led, as already remarked, to see in the difference of diet a considerable latitude in the ultimate sources of heat. This contrast in the usual quality and quantity of the food of the arctic and tropical races offers so inviting an explanation, as to tempt the mind into resting satisfied with it as sufficient to explain every case; but we must not allow our imagination to be thus wholly carried away.* Fully appreciating the fact that a material difference in the diet does exist, and must tend to the development of very different degrees of heat, we shall find that it will not account for many cases, especially such as I have described. During weather in which the outgoing of heat could not possibly approach near to what it is in England,—in which, indeed, every avenue for it was almost closed,—I have known many persons to retain their appetite, and to eat quite as heartily as the generality of persons in England, and of food quite as rich in carbon and hydrogen as any which forms the usual diet of the people of this country.

Many of the natives also ate heartily of rice, pease, and butter, in large quantity, which together form the diet of those whose means are sufficient to command them. Now such a diet, next to one of pure fat, is as rich in the elements which yield heat in combustion, as any that could be partaken of. While, then, it is quite true that a high temperature, especially such an oppressive atmosphere as that described, tends to lower the appetite considerably, and while there cannot be a question but that restrained eating at such times is a safer and more

* Appendix III.

wholesome course than the opposite, yet the fact must not be overlooked, that very many persons continue to eat largely, and thereby take in a quantity of fuel, which, unless our theory of animal heat be modified, would destroy life in a few hours; and in the case of none is the diet reduced so far as in such atmospheres would be necessary to prevent death in a few days, if its elements were evolved with as large a development of heat as they might yield in ordinary combustion.

Are we not then led to this conclusion, that while we look to an union of carbon and hydrogen with oxygen as the source of animal heat, and while we know that that union must bear a constant proportion to our food; yet, when we often see this union to take place to an extent that would generate a far larger quantity of heat than could, under the peculiar circumstances, by any possibility be got rid of—are we not led to the conclusion, that the character of the union is not to be viewed as identical with that in combustion, in which a given quantity of carbon and hydrogen, *when their source is the same*, always yields an equal amount of heat? Are we not driven, indeed, to the conclusion, that when the union takes place under the control of the vital powers, the manner or intensity of it may be so regulated as to yield very different quantities of heat at different times? Thus, when much animal heat is wanted, a given quantity of animal fuel may be made to develope the full amount of heat which it would in ordinary combustion. On the other hand, when nature is compelled to get rid of this quantity of matter in the same form of carbonic acid and water, but could not tolerate a large development of heat, the union may be so effected as to generate little heat.

In very oppressive weather I once saw a gentleman make the following breakfast, which he stated to be his daily practice, and that he also ate heartily at dinner. His breakfast consisted of eight eggs, a large plateful of fried fish, with several replenishings of boiled rice and melted butter, concluding with several slices of pork; his appetite, in fact, verged towards a bulimy. Though in the end it brought on gout, which proved fatal, this gradual result was no explanation of the daily operations of nature. Much the greater part of this excessive quantity of digested fuel must, we know, have passed off by the lungs. Had its resolution into carbonic acid and water developed a proportional quantity of heat, it would, finding almost no exit, have killed him in a day. It was impossible not to draw the conclusion, that nature must possess the power of effecting the union in such a manner as to moderate greatly the generation of heat.

In our own country we may, not unfrequently, meet with men of large person, and therefore large outer surface, who are small eaters, and yet never chilly from want of heat; and again with others of smaller persons, who are larger eaters, and at the same time always chilly, and compelled to wear much clothing. Quite admitting that the contrary is the rule, and that such cases are the exception, we are yet bound not to pass by these exceptions. To say that the state of either is unnatural or unhealthy, does not at all explain their continuance from day to day, and even from year to year, in that state. In another case, familiar to us all by personal experience, the same fact is proved. Upon a sudden change from cold to warm weather, we feel at first much oppressed, but less so after a few days, and we say that we have got accustomed to the heat. So again, on a sudden setting in of cold weather, we are pinched with cold, but in a short time we feel it little, and we speak of having grown accustomed to it. Now, there is by no means any determined change of food corresponding with the change of temperature. In the case of heat there is, to a certain extent, as already admitted; but our appetite does not improve by being pinched with cold in England in any certain degree, and we cannot overrule the exit of heat. What, then, is our growing accustomed to the warmth or cold, but, in part at least, a power of, by degrees, modifying the quantity of heat we draw from the same elements? So with respect to clothing: a person may continue his light summer dress on into weather far colder towards the close of the year, than that in the spring in which he could venture to resume it. In fact, we know how much more often cold is taken by suddenly leaving off warm clothing, than by not taking to it soon enough. It would be idle to assert that a man eats more in the autumn than the spring as the cause of the difference. There is no sudden change of diet corresponding with these results. They are to be attributed to modified developments of heat from the same materials. But, after all, the tropical cases I have instanced are too prominent for exception to be taken to them.

Again, when we reflect upon the phenomena of combustion, we are compelled to admit that we know very little regarding the source of the heat generated, even when the action takes place under our sight, and is the simple case of the union of two elements, as in the combustion of charcoal. In more complicated cases, as where some *compound* of carbon undergoes a destructive resolution into carbonic acid and water, we do not by any means find the heat evolved to be proportional to the quantity of carbon and hydrogen present. Thus, the heat produced by the combustion of alkohol or oil, not only readily maintains the action, but is set free in large quantity; whereas a quantity of a vegetable acid, though anhydrous, which shall contain an equal amount of carbon united with oxygen and hydrogen, upon being resolved into carbonic acid and water, is so far from being able to yield a like quantity of heat with the oil or spirit, that it would require a part of their spare heat, or heat from some other quarter, to aid it in maintaining its combustion. Thus very little heat is derivable from the combustion of most of the vegetable acids.

Again, if we mix together sugar and nitre, in such proportions that the total quantity of carbon, hydrogen, and oxygen present shall exactly correspond with the quantity of these elements in the given quantity of vegetable acid, we shall find our mixture not only highly combustible, but to yield so large a quantity of heat that its combustion will present the phenomenon of deflagration; yet the gaseous products will be the same as from the decomposed acid, with the exception of the nitrogen of the nitre, which we have reason to suppose rather tends to lower the heat by absorbing a portion in assuming the gaseous form. Not to multiply instances, we may affirm, that our knowledge of the nature and source of heat does not authorize any such assumption as that, whenever a given quantity of carbonic acid and water are formed, the total heat developed must be equal under all circumstances, whether the union be quick or slow, or from whatever compounds the carbon and hydrogen are derived.* So far as our observation extends, it is not so ; and if it were, we should then find our theory of animal heat to fail us in the cases above alluded to, where a copious production of carbonic acid and water takes place, while there is no possible outlet for the heat, should its quantity be estimated by the rule of ordinary combustion.

Furthermore, if the heat developed bore an absolute relation to the carbon discharged, a farinaceous diet would be far more heat-producing than an animal, with the exception of fat. A pound weight of any farinaceous grain contains far more carbon than a pound of flesh ; according to some analyses one half more. Consequently, we should find the rice diet of the Bengalee, and the wheaten diet of the native of Oude and of Dehlie, all extremely hot climates, to be far more heating than the flesh diet of an Englishman ; whereas we know the very contrary to be the fact, and that our appetite for flesh diet, by a wise provision, declines as the temperature rises. Let it not be said that the difference of carbon is made up by more meat being eaten than

* Appendix III.

grain; for we know that the quantity taken is less, when an animal is substituted for a farinaceous diet, as of bread or rice. In this instance, then, we are driven to the same conclusion, that the heat developed bears no absolutely fixed proportion to the elements combining, when the process takes place under the control of the vital powers.*

The summary of the observations under the present head is as follows: —

1. That water is unquestionably formed in the animal system as well as carbonic acid; therefore, that hydrogen as well as carbon is to be viewed a source of animal heat, and that we have no reason for concluding that there are any other sources.

2. That since by far the greater part of the food is thrown off from the lungs, we must look closely to the quantity of diet as the source *in general* of the different quantities of heat required in different countries; and that we find, in the case of the inhabitants of intensely cold regions, both the quantity and quality of the diet to bear a close relation to the very large quantity of heat required to maintain the animal temperature in those regions; while, on the other hand, the fruits and acidulous diet, for which there is an inclination in the tropics, may with good reason be considered to yield less heat in passing into carbonic acid and water, since a large portion of the oxygen requisite is already solidified in such vegetable principles, and, being in union, is less likely to generate heat.

3. But while a large quantity of highly carbon-

* Appendix IV.

aceous food does appear necessary for yielding a great quantity of animal heat, as in the arctic regions—while, indeed, we are assured that nature cannot draw more than a certain quantity of heat from a given amount of fuel, we have above seen overwhelming evidence of the fact, that the development of heat by no means keeps pace with the amount of dietetic fuel. It is any thing but true that the animal heat bears a constant proportion to the carbon and hydrogen of the food.* It is a fact that, under the control of the vital powers, flesh will yield more heat than rice; though, measured by the rule of combustion, the latter is far stronger fuel. It is a fact likewise that flesh, grain, and all kinds of food, though they cannot be made to yield more than a certain quantity of heat, may be and are often resolved into carbonic acid and water, in such a manner as to yield much less. We must guard ourselves against ultra-chemical views, and bear in mind the near resemblance of vital to galvanic influence. Little as this latter agent is as yet under our control, we have already attained the power of making it, *with the same amount of chemical action*, develope a great deal or very little heat, according to the nature of our battery. This is a very important consideration. Hence I cannot fall in with those who would trifle away Sir B. Brodie's experiments. Though a warm advocate for Crawford's theory as modified by Lavoisier, I do not believe it complete without a further modification, pointed out by such experiments, and indispensable to make the theory agree with climatorial facts.

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Thus guarded, our theory of animal heat will be found to answer to every circumstance of animal life. Otherwise, every step we take in Asia, not to mention Europe, presents to us instances of its failure as glaring as the tropical sun which produces them when it narrows every outlet of animal heat.

CHAP. III.

Our second inquiry is, Is the matter which yields the carbon and hydrogen evolved by the lungs derived from the waste of the corporeal fabric itself, or directly from the food in the form of chyle in the blood, or from both these sources? And is the whole food, with the exception of the small excrementitious portion, convertible into the animal fabrics, or only that portion of the food which corresponds with the fabric in its elementary composition?

A right view upon these questions is of great importance to the advancement of our knowledge; without it we cannot but form confused notions with respect to the most important functions of animal life, especially those connected with digestion, assimilation, and the development of heat. The two questions, though apparently distinct, will be found closely dependant on each other. They are therefore treated under the same head.

In the first place, it is scarcely necessary to revert to the fact that the whole of the carbon and hydrogen which are resolved into carbonic acid and water, and pass off by the lungs, must ultimately be derived from the food.

Now with respect to this portion of the food yielding the animal heat, one of three propositions must be maintained. Either, first, that before it is carried off from the system in the form of carbonic acid and water it does not ever form part of the animal fabric; or, secondly, that it always does; or, thirdly, that it sometimes does form part of the body before being employed for maintaining the animal heat, and at other times is derived directly from the food in the form of chyle.

That the first supposition would not be correct, we may at once decide from the case of persons ill with fever. For many days, often no food is swallowed; the body undergoes rapid emaciation; the bowels are confined, and could not, indeed, carry off much; and at the same time the animal heat is not only abundant, but much above what is natural. We learn from this that the greater part of the flesh lost must have passed off by the lungs; and that its resolution into carbonic acid and water was the source of the heat, for there was no food whence it could be derived; and the rapid emaciation corresponds with the undue development of heat.

Again, in the case of a person in health commencing a course of severe exercise, we cannot doubt that the loss of flesh is closely connected with the increase of the circulation of the blood, and the consequent increase in the volume of the respired air, and also with the increased production of heat, which corresponds with the other phenomena.*

Lastly, we know that the absorbed matter of the body must, in part at least, pass off in respiration, and by the skin; and must therefore, to that extent, supply elements available for the production of heat.

These cases are quite enough to satisfy us that the

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materials producing the heat may be, and often are, yielded by the fabric of the body, both the fat and the flesh;—the fat being at once resolvable into the gaseous discharges of the lungs; and the flesh yielding besides, nitrogen, which may sometimes pass off by the lungs uncombined, and at other times by the kidneys in the nitrogenous principles of the urine.

The second view is the converse of the former. Since the materials yielding animal heat are in certain cases wholly derived from the dissolved fabrics of the body, the next question is, Have they not always this source, and no other? or, in other words, Is it not necessary, before any portion of the food can pass off by the lungs, that it should have been assimilated into corporeal substance?

The question is one of no small importance to a right knowledge of the functions of many organs of animal life, and to a clear insight into the effect of climate on the animal frame.

So long as we consider the nutrition of the body to be the main duty of the organs of life, and the maintenance of the animal temperature one of secondary importance, we are likely to be taken up with the notion, that for animal warmth fuel can only be spared from the absorbed fabrics; that it would be a waste of food to use it only as fuel.

It is probable that such an impression as this has had its influence in leading most physiologists to consider the source of animal heat to be either the waste fabrics alone, or, in addition to these, such refuse principles in the chyle as cannot be assimilated.

To arrive at a right conclusion, let us first keep well in view the fact, that it is a duty of the organs of life, second to none in importance, to maintain the animal temperature always at the point ordained, and that under outward causes operating powerfully to disturb, and, in general, fatally to reduce it; that it is more momentarily necessary than nutrition itself; that a full degree of internal warmth is an essential condition of living fabric; that we may often allow the quantity of the fabric to decrease greatly from imperfect nutrition without damage to life—without even permanent injury to health; but that a very small diminution of the animal temperature cannot be allowed to take place without a fatal result; hence, doubtless, when the supply of food is cut off, nature employs the useful fabrics of the body for keeping up the animal temperature, though there can be little doubt that she husbands them as far as possible, by lowering the respiration*, as before remarked, and the perspiration to a minimum. She generates, when at such a cost, as little heat as is compatible with the maintenance of the animal temperature†, and reduces the excretions, which would carry it out, especially the perspiration, to the least possible.

There is, then, in the paramount importance of the animal heat, quite enough to assure us that food cannot be more advantageously employed than in maintaining it; provided the waste materials of the body do not supply enough of the requisite carbon and hydrogen.

* Appendix I.

† Hence, though the inward temperature must be kept up, the surface may be distressingly chilly from the minimum production of heat.

We must not, however, rest satisfied with this conviction alone, that the materials for the gaseous discharges from the lungs *may*, in part, be derived directly from the food. The following consideration will show that they constantly *are*.

The renewal of the bodily fabric has no necessary connexion whatever with the development of heat. The latter is merely an attendant on the renewal, since the waste materials thrown out of use are, for the most part, so transformed that their two chief elements shall yield, by uniting with oxygen, animal heat.

In their very nature, the exchange of the fabric of the body and the production of heat are wholly distinct wants of the animal system. Often a quick renewal of the animal fabric is required, and but little development of heat; and, on the other hand, a very moderate renewal, but a very large quantity of heat. We must not be misled, by the concurrence of the two in some cases, into supposing them to be always mutually dependant and proportional. We must not think that because a larger waste of the bodily fabric and a larger development of heat take place together under exercise, that the one is inseparable from the other.*

Without entering here into a detailed examination of the effect of exercise, we may perceive that the increased muscular action gives rise to a general increase of vascular action, and of consequence of all the functions of the vascular system. Hence, a quicker exchange of the bodily fabric, and from it a larger supply of the heat-producing elements, and

* Appendix VII.

an increased respiration to introduce into the system oxygen in proportion. Without in this place entering further into, what we may believe, the particulars of the process, it is at once evident that the change of fabric and the heat are merely so far connected that the latter is *in general* increased whenever more carbon and hydrogen have to be got rid of, and is therefore increased in the present case.

Prosecuting our inquiry beyond the case of exercise, we shall find numerous instances in which the resolution of the fabric of the body, and the production of heat, bear no relation to each other. Thus there is strong evidence that the change of the bodily fabric is much quicker in India than in England. The quickness of the growth and maturation of youth in India; the surprising readiness with which the natives gain and lose flesh from causes which would not affect an Englishman; the copious flow of milk in the case of females; and the quickness with which wounds heal and scars disappear, together with other facts, establish the fact that the animal fabric with them undergoes a far quicker exchange than with us. But is their development of heat proportionally larger? The very contrary. For many months of the year there appears little need of the production of animal heat, and no very ready means for its dissipation: in some weather, apparently no outlet for it. In intensely oppressive weather, I have seen native rope-dancers and jugglers go through muscular efforts not only excessive, but the movements rapid and long continued, during the exhibition of some of their extraordinary feats, in weather, indeed, in which the atmosphere could carry off little heat either by conduction or evaporation. All these are proofs that the changes of the fabric and the development of heat are distinct operations.

Again, in England we have the opposite condition. Here the cold often calls for a large supply of heat, when there is no exercise going on, and evidently no rapid exchange of the fabrics ; as, for instance, in the case of persons riding in winter on coaches, and in open railway carriages, some of whom we see generating so much heat as to require little clothing, yet with no conceivable change of their fabrics in proportion.

Lastly, it would be beyond measure improbable that, for the purpose merely of supplying materials for supporting the animal heat, so laborious, circuitous, and unintelligible a course should be taken by nature, as that of building up the minute and curious fabric of the body, and then pulling it to pieces ; yet such must be the case if we think that no food can pass out by the lungs but such as has gone the circuit of forming a part of the body, — if, confining our view to one particular case, that of the body under exercise, we imagine, because the corporeal change and evolution of heat are in that instance contemporaneously increased, they must at all times proceed together, so that no heat can be formed but by a dissolution of corporeal fabric.*

When the subject is duly considered in all its aspects, we can hardly fail of coming to the third conclusion, that the materials yielding the animal heat sometimes form part of the fabrics first, and at

* Appendix V. and VII.

other times pass at once into carbonic acid and water. Thus, we must in reason conclude, that when the ordinary waste of the fabrics is sufficient to supply all the material needed as fuel, the animal heat may, during such time, have that source alone; when the waste of the fabrics is much increased, as during unwonted exertion, the production of heat will increase, on account of the larger oxydation going on of carbon and hydrogen removed from the system by the greater activity of the circulation. Lastly, when the dissolution of the fabric is not proceeding at the pace requisite to keep up the animal temperature, then materials to supply its fuel are derived directly from the chyle. This last, we may believe, is the usual state of things, especially in cold climates. In some, as we have seen, there is strong evidence that by far the larger portion of the food has that destination alone, passing into chyle, and thence into carbonic acid and water, to yield the large amount of heat requisite; while the little change of fabric in a very cold region makes but a limited demand for building materials on the chyle, and supplies little waste for fuel.

We shall find a strong confirmation of these views in the result at which we cannot fail to arrive respecting the next division of our inquiry.

Is the whole food, with the exception of the small excrementitious portion, convertible into the animal fabric, or only that part of the food which corresponds with the fabric in its elementary composition?

From what has been said above, it is evident that a large part of the food must often pass off by the lungs without first entering into the composition of the body, as in the case of the large diet of the Greenlander, which, as before stated, we cannot imagine it either reasonable or possible should all be built up as a part of the body, and an equal quantity pulled down again as the only way of yielding the elements necessary for heat. It would involve us in the conclusion, that the soft parts of the body were renewed every month in the case of the Greenlander, whereas it is probable the renewal is much slower than in the tropics; and it would bring before our view the elaborate process of the formation of fibres and tissues, vessels and nerves, all carefully and minutely deposited, simply as a stock of fuel.* There can then be no doubt that much of the food may never enter into the composition of the body.

But though of the food much does not enter into the composition of the bodily fabric, it does not follow that if it were wanted as nourishment it could not. It is not all assimilated into the fabrics when taken in large quantity, because no such rapid renewal of the fabric is required for the preservation of the system—because it would be exhausting to the powers of life, were the renewal carried on so violently.

In the case where little heat is required, but rather a quick renewal of the fabrics, as in the tropics, to what extent is the food convertible into the animal fabrics?

This is an interesting question, the decision of which will influence our judgment upon many others.

* Appendix VII.

When the food is solely of animal matter, being of like nature with the living body, or at the least containing all the same materials, and closely, if not exactly, in the same proportion, we may readily believe it to be all convertible into the bodily fabrics, if need should so require.

Let us not, however, imagine because all of it *might*, therefore it *must all* be so employed.*

But in the case of vegetable food, at what conclusion are we to arrive? We here find none to comprise a moiety of compounds containing even the same elements as the body; few consumed as food by man, to contain more than one fourth part of their weight of nitrogenised products, while many amylaceous grains, as rice, are nearly all starch, the nitrogenised principles in them not amounting to the tenth of their weight. So also in the food of many beasts, as in grass, the nitrogenous matters are equally scanty. Are none but these last convertible into the bodily fabric? Have the vital powers the ability only to separate from the food ready-made matter, or matter nearly similar to the fabrics, or could they not bring about such an union of other principles of the food with nitrogen, modifying the proportion of their elements in any necessary degree, as to resolve them into proximate animal principles allied to the bodily fabrics, provided a supply of nitrogen could be found?

Whether the vital power does or not resolve dissimilar principles into matters allied in composition to the body, we can hardly conceive that it could

* Appendix V.

not, from want of force to control the affinities of the elements. Such a notion would assign to the highly strenuous vitality of an animal less vital influence than is possessed by the feeblest herb,—less than is enjoyed by any ephemeral flower leaf.*

Again, granting that the vital powers could transform dissimilar into principles similar to the bodily fabric, it may be contended by many that it does not, since the element nitrogen is absent, and there is no recognised supply of it by which such articles as fecula, sugar, &c. could be brought into similarity with animal principles.

If this be so, then must animals subsisting on vegetable products find their nutriment in whatever nitrogenous compounds already exist in such food; and they must consequently resolve nearly all the rest into carbonic acid and water, and discharge it by the lungs, since the proportion discharged as excrement from the bowels is, in the case of grain, very small. We shall soon find such views to be wholly inadmissible. †

We may learn from the analytical inquiries of others, so much at least respecting the food of herbivorous animals as will enable us to draw a conclusive argument from it in the case of the horse. The nitrogenous principles, in the ordinary quantity of hay which a horse eats, do not afford him materials having all the same elements with those of his body in quantity equal to what is afforded to many a man partaking largely of animal flesh. If we were to select an animal whose excessive

* Appendix VIII. † Appendix V.

daily exertion called for a frequent renewal of its fabric more than any other, it would be the horse. The quickness with which, between the action of food on the one hand and labour on the other, that animal may be seen by the one to gain, and by the other to lose, not fat only, but muscular flesh also, may assure us that the great exertions, and consequently accelerated circulation of this animal, when in full work, must involve with them, as shown by the quickened respiration, a large renewal of its bodily fabric,—not only large on account of the magnitude of the animal, but even specifically large. We cannot then for a moment believe that the whole absolute amount of its nutrition equals only that of a man.

Again, can it be imagined that, in order to gain the miserably scanty supply of sustenance (if estimated by the little nitrogenous principles ready formed in its food), the horse has to take in a prodigious quantity of heat-producing carbon and hydrogen in the form of the bulk of its food?

Can we entertain the notion that herbivorous animals are thus burdened with an enormous proportion of fuel, twenty times the weight of the spare nutriment the hypothesis would only assign to them? If we cannot clear our view on this point, on account of the cold and foggy atmosphere in which the horse has to labour in England, which may confuse and delude us into thinking that a production of heat may be needed exceeding twenty fold his production of flesh, even under heavy labour, let us then follow an *English* horse to the tropics. We shall there find him, though the native of a cold climate, able to endure the hottest weather in a surprising manner. We shall see him in full action for hours in an atmosphere as hot as his blood in the shade, and in the sun opaque bodies around him verging towards a boiling heat. There the matter for surprise is, how his surfaces, cutaneous and pulmonary, can prevent the ingress of outward heat. Our very sympathy for the animal revolts against the notion of his having to gain sustenance at the torture of the enormous production of heat, if measured by the very large quantity of carbon and hydrogen, when compared with the nitrogenous principles of his food; the nitrogen in either oats or hay not amounting to more than the fortieth or fiftieth part of the former, and the seventieth of the latter.

If we compare with his state that of the dog exported from England to India, the evidence against the hypothesis becomes, if possible, stronger still. The dog, if fed on flesh alone, ought, according to the hypothesis, to obtain its nutriment with a minimum production of heat; in fact, with no more than would unavoidably attend the oxydation of the waste animal fabrics of which the system has to be relieved. Such an animal ought to bear well a transportation to a hot climate; while the horse ought to perish, on account of its nutriment being accompanied with so large a quantity of heat-producing, and, according to the hypothesis, non-nutritious matter. But facts are directly opposed to this. The horse, instead of perishing, as he inevitably would, were the hypothesis correct, bears the climate, as already observed, remarkably well; while it is very difficult to keep English dogs* alive in India, even by the most careful protection from the sun.

Again, if we turn our attention to the more important case of man, we shall find all evidence falling on the same side. In the diet of the Hindoo, of the Bengalee especially, the nitrogenous principles are in so small quantity as to render it incredible that they alone could supply the necessary nutriment. In the rice and butter, upon which exclusively thousands live, where is the sufficient supply of prin ciples ready prepared for assimilation? Can we believe that, in a country where, as before shown, the bodily fabric undergoes a quicker change than in our own, the materials for that change should be contained in scarcely an appreciable quantity in the food? Furthermore, why should such a diet be instinctively selected by intertropical nations, if nearly all of it were of a nature fitted only for resolution into carbonic acid and water, and therefore for only producing heat in climates where animal heat is so sparingly wanted,—where, during many months of the year, it is absolutely a burden?

And, on the other hand, how is it that a flesh diet is by instinct sought by the nations of Europe, and invariably preferred, when procurable, to one exclusively vegetable, if the latter were so much

* Let not this be laid solely to the small cutaneous perspiration of the dog; for in this respect it does not differ from the various beasts of prey, of which the tropics are the favorite region; and I have already shown how limited exhalation from the surface often is in certain states of the atmosphere.

more heat-producing in proportion to its nutritive power?

It were a superfluous task to expend time in pressing more arguments. We find as *a fact* that the farinaceous diet of the Hindoo is not more, but much less, heat-producing than our own; while the quicker change of his bodily fabric assures us that his food is well suited to supply the means for effecting it.

Again, however opposed to any of our theories, it is a fact that a flesh diet is not sought, but eschewed, in very hot weather and climates, and is evidently more heat-producing than a farinaceous one containing a much larger quantity of carbon.

We are, then, brought to this conclusion, that the nitrogenous compounds of such a vegetable diet do not, and cannot, supply* all the nutriment extracted from such diet; nor can the very large quantity of

* Professor Johnston, in his instructive little work on Agricultural Chemistry, does not omit to notice the fact, that to supply nutritive nitrogenous principles, equal to those in $1\frac{3}{4}$ lb. of bread and 6 ounces of cheese, no less than 4 pounds of rice must be eaten: perhaps 5 pounds of Bengal rice would be nearer the quantity. And yielding to the opinion now prevalent, that only principles containing nitrogen are nutritive, he proceeds to state (p. 236.), upon report, that the natives of India distend themselves with great quantities of rice. The observation requires explanation. The quantity of rice in a meal of large bulk is after all small. This grain swells greatly in boiling, especially in their light manner of cooking it. The weight of the raw grain any of them consumes is very moderate, when we consider it is their only food. It would not afford them near enough of nitrogenized principles for their nutriment; or it would overpower them with heat-producing elements, if they had to consume these to gain the former.

non-nitrogenous matter be employed only for producing heat.

This point being fully established becomes itself the foundation of an argument, which will open to our view highly interesting, and, I conceive, demonstrable facts, giving to atmospheric nitrogen a place of importance it has never been allowed, and removing an accumulation of difficulties besetting every hypothesis at present adopted.

CHAP. IV.

The argument of the preceding chapter directs us to the third question for consideration—Is the nitrogen of the atmosphere of no other than passive service as a diluent of the oxygen, or is it actively engaged in the transformations which take place in the body?

In the case of animal diet we have had little difficulty in perceiving that while it affords an abundance of ready-formed principles for the nutrition of the animal fabric, only so much of them will be thus employed as the exchange of the fabrics going on at the time requires. The rest, together with the absorbed or waste elements of the fabric, will be so resolved as that the greater portion of its carbon and hydrogen shall unite with the oxygen of all these fresh and waste principles, and with the oxygen introduced by respiration; and the union shall be so effected as to form the binary compounds of carbonic acid and water, these being admirably suited for discharge by the lungs and skin, since they pass off gaseous, invisible, and inodorous. A small quantity of the carbon and hydrogen is retained to form, with the waste and surplus nitrogen, a compound, not very volatile, but soluble in water, and therefore convenient for being carried off in the form of urinary salts and other products. Thus compounds are got rid of which could not, for many reasons, pass off by the lungs; amongst which are their fetor, and want of sufficient volatility. At the same time, if the necessary carbon and hydrogen for resolving all such nitrogen into urea cannot be spared, will any one venture to affirm that such free nitrogen does not, and cannot pass off by the lungs, and perhaps by the skin also? The strongest reasons will appear to show that this does happen.

Next, with respect to vegetable diet. Here the main question falls into place, namely, as to whether the nitrogen absorbed in respiration is merely a passive diluent of the accompanying oxygen, or whether it is, like the latter, actively engaged in the transformations going on in the system?

We find it to be, according to the most approved experiments, absorbed, as well as the oxygen, into the blood.

In the blood it travels (for any reason we know to the contrary) with the oxygen to the point of action, wherever that really is, which I do not here discuss. It is admitted on all hands to make the circuit of the capillary system; for it *appears* again in the venous blood, and is discharged into the cells of the lungs, according to received opinions.

What then are the grounds upon which the nitrogen should be denied an active part in the assimilations and resolutions going on in the system?

When we consider that nitrogen is an element entering largely into the composition of the animal principles, which are the products both of assimilation and resolution, and when we admit the evidence that it is actually and freely absorbed into the blood as well as the oxygen, and makes with it the round of the circulation, the following are the only grounds upon which it would be possible to deny that it may, or reasonable to doubt that it does, take part in the transformations in question.

First, if it can be shown that the vital powers are unequal to the task of forming animal principles out of vegetable principles by the addition of any element requisite for the transformation.

Secondly, if it can be shown that the nitrogen of the nitrogenous elements of the food is sufficient to meet all the demands of the system for nitrogen ; or,

Thirdly, if it can be proved, that the nitrogen discharged by the lungs is the very same with that absorbed so recently before as to have had time only to circulate with the blood.

It is readily admitted that any one of these positions, if established, would be fatal to the proposition that the nitrogen which enters by the lungs performs a part in the animal chemistry.

First, can it be shown that the vital powers are unequal to the task of so modifying the composition of vegetable principles, by the aid of other elements, as to resolve them into animal principles?

That the vital powers are so feeble we have no proof whatever. We know them to control and overrule the most powerful affinities. In the case of plants, as already instanced, the affinity between carbon and oxygen is made to bow before their force ; and the very fact in question—the union of nitrogen with other elements—is effected even by the vital power of vegetation, and proximate principles are produced into which this element enters largely.

If we look to the higher and more powerful vitality of animals *, we find, in the case of the various urinary products, that nitrogenous combinations, which had no existence in the food, are brought about entirely under the direction of the vital powers; and this merely for the purpose of giving to waste materials the convenient and compact form of substances soluble in water, and parted with as urine. Thus the highly nitrogenous principle urea is a formation constantly going on as a process of animal chemistry. And again, the much larger formation of the principles of the bile, containing little nitrogen, is equally a process of the living chemistry: whether they are formed in the liver, or only separated there from the blood, having been formed elsewhere, they are new compounds formed in the system under the control of the vital powers.

The natural and reasonable conclusion from all this evidence (and much more might be adduced) is, that the vital power is undoubtedly able to effect any such modifications of the proximate principles introduced as food as the exigencies of animal life may require. This leads us to the second question —

Are the nitrogenous principles of vegetable food alone sufficient, under all circumstances, to nourish the fabric of animals feeding exclusively on vegetable productions?

This question has been already sufficiently discussed to decide the point. It has been seen above that the azotized principles of vegetable food cannot be viewed as those alone devoted to the nutrition of

* Appendix VIII.

the body. But as the physiological opinions of the day are taking that turn, it becomes necessary to consider the question more fully.

The interesting discovery of the azotized vegetable principle proteine by Mulder, proving that vegetable food contains, ready prepared, a principle closely allied to the fabric of the body in its composition, has given rise to the inviting opinion that the whole process of digestion and assimilation is a simple separation of the ready-made azotized principle in the food, which it is supposed supplies all the nutriment of the body. Prior to the recent exclusion of all non-azotized principles from the theory of nutrition, Dr. Prout well discerned the necessity of explaining how these principles of the food should become azotized so as to afford nutriment. He supposed that the nitrogen requisite is afforded by "a highly azotized substance (organized urea?) secreted from the blood, either into the stomach or the duodenum, or into both these localities." It is, indeed, very probable that this substance is the *immediate* source of the nitrogen which may serve to transform principles wanting this element into such as shall be fitted to form part of the animal body; but whence is the nitrogen *ultimately* derived? Since it is wanting in this kind of food, it must be derived either from the effete matter of the body, or from the nitrogen of the respiration. Let us suppose it is not from the latter. If it be from the former, then must we suppose that the same nitrogen again and again enters into and is separated from the animal fabric, forming a union with every fresh parcel of non-azotized food. If we once admit as a fact, that some effete principles are thus brought into use again, no reason appears why all should not — why an everlasting circulation should not go on, of resolution and combination afresh of the same elements ; and consequently why there should be any necessity for taking food for any other purpose than the production of heat, — why animals might not, so far as nutrition is concerned, live upon their own effete matters. We are compelled then to seek some fresh source of nitrogen. Moreover, even if we could assent to the notion that effete principles are renewed into living flesh, it is obvious, for them to supply all the nitrogen, none must pass out of the body, as it does copiously in the urine, otherwise all that is in the system would in time be consumed ; for the rice diet of the Hindoo does not yield proteine enough to make up for the azote carried off in the daily discharges. But even if this difficulty could be explained away, we must surely refuse admission to the notion that effete principles are employed over again in nutrition, — and it is probable this notion has not been adopted ; yet it follows inevitably upon the evidence that nourishment is derivable from non-nitrogenous principles, unless we admit that the nitrogen may be yielded by respiration.

That vegetable principles not containing nitrogen can and do become assimilated, we have had already unanswerable evidence. The food of the horse, and other herbivorous animals, would otherwise, in a tropical climate, present to our view an arrangement so inconsistent with the wise provisions of the Creator, and with all observed facts, as to be altogether incredible.

To acquire for itself the nutriment only of a man, this large and laborious animal would have to consume a vast quantity of matter capable of being turned to no other account than that of producing heat, which in such a climate would, in so large a quantity, destroy the animal; whereas the horse suffers remarkably little from tropical heat, even when imported from England, and must therefore generate heat moderately; while the dog, which living upon animal food ought, according to that view, scarcely to feel the climate, soon perishes by the heat. Many other similar proofs may be adduced: let these suffice to assure us that the nitrogenous principles sparingly diffused in the food of herbivorous animals is but partially a source of their nutriment, and that the non-azotized principles which form the great bulk of them, are not solely employed for generating heat, which, even in the quantity unavoidably developed in the dissolution of the effete elements of the fabric, is in the tropics a source of oppression; but that these bulky non-azotized principles supply mainly the materials by which the animal fabric is renewed: and let us entertain the same consistent opinion respecting the food of man. His food of starch (rice) could never give him proteine enough to supply the exchange of his fabric in the tropics, which is not less, but manifestly greater, than here, unless accompanied with an immense mass of matter, the resolution of which into carbonic acid and water would serve no object but that of producing a quantity of heat destructive in such a climate. The Hindoo does not eat his meal of rice to burden himself with an extra quantity of heat in his oppressive climate, but undoubtedly to convert the fecula of which it is nearly all composed into materials for renewing the fabric of his body.

From all that has been said, the conclusion seems inevitable that nitrogen, from some source, must be found for effecting the necessary changes in those portions of non-azotized food which assuredly are convertible and converted into animal fabrics: furthermore, that no other source is presented to our view than the nitrogen of the respiration. The only remaining ground upon which a doubt could well be rested against this source for the nitrogen required, is to be found in the third question —

Is the nitrogen discharged by the lungs proved to be identically the same with that absorbed so recently before, that it has had time only to circulate with the blood; or, in other words, can it be proved that none of the nitrogen absorbed is retained for any length of time in the system? Of this we have no proof whatever.

The only inference against any requisite portion of the absorbed nitrogen remaining for an indefinite period of time, forming part of the animal principles, is drawn from the quantity discharged corresponding, or appearing to correspond, with that absorbed, and from its returning from the blood in an uncombined state. But the supposed fact that the quantity of the nitrogen returned from the circulation corresponds exactly with that absorbed is not only without proof, but many experiments, those of Professor Edwards especially, tend to show that there often is an observable loss of nitrogen. This is very notable, for it points to the fact that the atmospheric nitrogen is retained in the system; and it would alone decide the question sought, were not the indispensable need of nitrogen to convert many vegetable diets, itself conclusive of the fact, that the nitrogen introduced in respiration must supply the quantity needed. If *any* part of the absorbed nitrogen can be detained, then is the point settled; and it will at once appear, that the nitrogen lost is in all probability only the *apparent* quantity detained. The whole might be detained, and a corresponding quantity given out, without any change in the expired air by which we could detect the exchange; for it will *appear* as if the *same* nitrogen all came back again undetained, whereas it might be that, which entered at some period long previous, having in the meantime formed part of the fabrics. In other cases more may be detained than is returned to the lungs: then only a loss will be observable equal to the difference; and it will seem as if this difference were all the nitrogen retained. And in other cases there may be more nitrogen returned than was absorbed. This would give rise to *an apparent loss of oxygen* from the inspired air, over and above that corresponding with the carbonic acid formed.

This calls upon us to revert once more to the important fact, that the air expired is no necessary index of that inspired. Being a mixture of a small part of the air just inspired (the rest having become resident in the chest) with a large quantity of the resident air, its quantity is no measure of the quantity of the air previously inspired; unless the experiment is carried through a large number of respirations, and with the caution, at the end, of ascertaining that the resident air has not been breathed out. Any one who will make the trial will find, at the termination of it, an inclination to relieve a feeling of constraint, which has been produced, by a deep inspiration or sigh. This proves that resident air has been expelled, in making the expirations balance in quantity the inspirations. But it is very probable that the two do balance each other, or are actually equal in the case of most Europeans, in whose food nitrogen is neither so deficient as to require the aid of that absorbed in the lungs, nor so redundant, from their living on flesh alone, as to make it probable a portion of the nitrogen of the food passes off by the lungs. Yet this may take place without our discovering the fact. When we find a loss of oxygen greater than corresponds with the carbonic acid formed, we judge of it by *an excess of nitrogen* appearing in a given bulk of air breathed out. But how do we know that the apparent excess of nitrogen may not often be an actual excess of it, instead of only an apparent one caused by a loss of oxygen?

Again, for the same reason there may be, when the demands of the system shall so require, the loss of oxygen so constantly observed over and above that which forms the carbonic acid discharged, and at the same time a loss of nitrogen, that is, less nitrogen returned than was absorbed. If this loss of nitrogen shall bear that proportion to the oxygen missing which exists in the atmospheric air, that is, if four times as much of nitrogen is detained as of oxygen (over and above that in the carbonic acid), then we may never miss either of them, unless by discovering the total quantity of the air expired, during a certain sufficient period of time, to be just so much less than the quantity of all the inspirations; for it is obvious that nothing will be manifested in the *quality* of such expired air, excepting an *apparently* larger production of carbonic acid gas, arising from some of the oxygen and nitrogen which would have accompanied it being both detained in such proportion as not to affect the quality of the remainder.

It is not contended that this exact detention of the gases in such proportions is likely often to occur; all that is insisted upon, is the fact that various quantities of each may be, and most probably are, detained, without the actual amount being discoverable: nor whether the change of proportions in the air exhaled arises from a deficiency of one gas, or an excess of another. Thus a man living upon a very hydro-carbonaceous diet, which lacks nitrogen, as fat pork, or rice and butter, cannot return to the lungs the whole of the absorbed nitrogen, but must detain a portion of it, which, after entering into the fabric of his body, may pass off in the effete matters discharged as urea; and at the same time he may require, for resolving into water the hydrogen of his food, some oxygen, besides that which unites with the carbon to form carbonic acid. If resident in a cold region, taking little exercise, and performing no labour, he will have not much wear of his body to renew, and therefore may not make any large demand upon the respired nitrogen; but, having to generate much heat, he will require a large excess of oxygen to unite with the hydrogen of the extra food he will consume. In his case, if we could exactly ascertain the absolute amount of all his inspirations for a day, and of all his expirations for the same period, we would find a loss of nitrogen in moderate, and of oxygen in large quantity; but if we make him breathe artificially out of one vessel into another, or by any other means try to ascertain the point by a temporary experiment, we shall, in all probability, lose sight of the total amount of loss; and if we form our judgment, as is usual, by the *proportional* qualities of the expired air, the loss thus indicated will be one of oxygen only, and that not the whole. A portion of the oxygen will veil, and be veiled by, the nitrogen which has disappeared. The two pairing off, as it were, in the proportion of atmospheric air, will neither of them be missed, for the proportions of the remainder will be the same as if they were present.

Again, the Hindoo bearer, running upon a rice and butter diet under the surprising load of three quarters of a hundred-weight, and sometimes a hundred-weight, for a distance of twelve, and often of twenty-four miles (each man being under the load half the time), this man will need a renewal of his fabric of no scanty amount. Though we might apportion to him the little fraction of proteine which such food contains, nature will, as he in his simplicity rightly believes, feed his flesh upon the starch and butter which form the bulk of his food; and we may be sure no more will be taken than is so needed, if he is running in the rainy season with the hygrometer not far from the dew point, and the thermometer at 90°; for the heat generated in throwing off the carbon and hydrogen of the large wear and tear of his fabric is quite enough to prove a serious task for him, poor man! to eliminate in practice, and for us in theory.

Can we doubt that this man will turn to account the free nitrogen circulating in his blood? Do not his diet, his labour, and his climate together prove it beyond a question? He then would, doubtless, show a deficiency of nitrogen in the expired air, some portion of that taken up by the lungs, and entering into the composition of the animal fabric, being, when resolved again, not returned to the lungs, but carried off in the urine in the form of urea, &c. Of oxygen there might be little loss, beyond that required to resolve into carbonic acid the waste carbon, there being in his food not any hydrogen unbalanced by oxygen, excepting that in the butter he consumed.

It has been stated above, that while the oxygen absorbed by the lungs proves itself to have been actively engaged in the system by returning united with carbonic acid, the return of the nitrogen uncombined has led to the general belief that this gas is inactive in the system; but let it be borne in mind that there are no gaseous compounds which it could have formed fitted to pass off in the breath, whether it united with hydrogen to form irritating and offensive ammonia, or with carbon to produce the pungent gas cyanogen.

As it could not pass off by the lungs combined in any manner which would be inoffensive and innocuous, we may well understand the cause of the nitrogen passing off uncombined. This we might readily believe of the vital arrangements, which are so admirably adapted to the purposes of life, although such a discharge of free nitrogen from its compounds were not observable in decompositions going on without the body. But we do find, not unfrequently, that a part, if not the whole, of the nitrogen of a decomposing compound, will pass off alone, while its other elements enter into union, forming binary or ternary compounds. Even animal decomposition may be so managed that free nitrogen shall pass off, and at the same time ammonia or cyanogen be formed; thus nearly corresponding with the vital arrangements which send back free nitrogen to the lungs, and pass off the ammonia and cyanogen (the latter resolved into urea) by the kidneys.

Without, however, involving ourselves by any means in the onus of determining the exact arrangements of the vital chemistry, we may stand to our general fact, that atmospheric nitrogen introduced in respiration may, and must, perform a part in the combinations going on. The only other channels for the introduction of any quantity of this element would be the skin and the saliva. The former would remove no difficulty: and it may possibly introduce some, though it would be superfluous to seek that channel when the lungs offer us an abundant source.

The little and uncertain quantity which the saliva could supply renders it hardly necessary to refer to it; moreover, being atmospheric nitrogen, it would not alter the question; and it would be much less reasonable to suppose nitrogen entangled in the saliva to supply this element, than that which we know to enter by the lungs; and it does so happen that the races who, of all others, require a supply of nitrogen, the rice-eating Bengalees, and all the inhabitants of that zone in the eastern seas, waste their saliva by constant spitting,—a habit induced by the prevailing use of the betelnut.

We find, then, the only ground upon which the too hastily assumed doctrine of the inutility of the respired nitrogen, except as a mere diluent of the oxygen, could have a tolerable foundation, namely, its return *apparently* unemployed, is of no real weight. It is no more than in the majority of cases would have to be expected; while the cases of men and animals living solely on vegetable food, if made the subjects of widely-extended experiment, would doubtless show an actual loss of nitrogen in the respired air,—a fact observed, indeed, by some experimentalists.

It being now generally admitted with Sir H. Davy, that the air in the cells of the lungs is absorbed bodily, that is, that the nitrogen enters the blood as well as the oxygen,—since experiment, so far as it is able, has decided this point, then, if we could, after all that has been urged in the preceding pages respecting the requirements of men and animals, doubt the fact that the nitrogen which thus enters the blood is employed in the system, so far as it may be wanted, we have left for our adoption only the following most improbable alternative. We are left to view the nitrogen absorbed, travelling with the blood into the minutest vessels, where the commerce of particles is actively going on, but no way concerned itself, where its associate oxygen becomes largely engaged, but rigidly avoiding its example; where we may affirm it is itself often greatly wanted, but regardless of the severest appeals of nature, even those of the rice-eating Asiatic; prying every where, but doing nothing; and at last returning to the lungs, after a long, intricate, and circuitous voyage, an idle and unprofitable intruder.

Is there, we might ask, any probability that such an arrangement exists in nature, where nothing is done in vain? But we have seen above evidence incontestable that the nitrogen circulates in the blood for no idle purpose; that unless we avail ourselves of it, our theories of nutrition and respiration are contradicted by facts on all sides.

Lastly, let us in this case, as in a former, bear in mind the near relation between vital and galvanic action. It is generally believed that every electric spark which traverses the atmosphere effects a union between its elements. In this way, indeed, the supply of nitrous principles to vegetables is in part accounted for. When these atmospheric elements, then, are circulating in the blood, and placed under the influence of the vital galvanism of the nervous system, might we not expect, from the above fact alone, a tendency to union, even though we had not all the preceding evidence of the urgent requirements of nature demanding of us the concession, as it were, at the peril of our lives?

CONCLUSION OF THE SUBJECT.

Let us now contemplate how it is that vegetable food contains a little nitrogenous matter only; why, if the non-nitrogenous principles can be converted into matter fitted to nourish the animal fabric, there should be any such ready formed in vegetable food.

Some acquaintance with the action of compound substances may enable us to discern a sufficient reason. We know that the presence of one principle in a mass often determines the formation of like principles with itself, or the progress of a determined and particular action in the whole. Thus the principle gluten, when introduced into a mass sufficiently moist to admit of the play of affinities, determines the action termed vinous fermentation: thus a little leaven leavens the whole mass. It is worthy of note that this gluten is the very principle in question, the nitrogenous compound sparingly diffused in vegetable food. Now, though the action within the body, neither in the digestive organs nor in the regions of assimilation, is of the nature of fermentation, yet we can little doubt that this principle gluten, acting under the control of the vital powers, may greatly favour the formation of like matter with itself, or rather with its main constituent proteine, since there is always present in the blood nitrogen supplied by the lungs as well as oxygen. Thus we may perceive that the nitrogenous principles of vegetable food are present, in part to feed as far as their quantity will go, and in part to determine the formation of similar matters from the more bulky non-nitrogenous substances of the food.

The formation of these very compounds of nitrogen in vegetables is closely analogous to this. We find that the soil must feed them with nitrogenous principles to favour the formation of such in their fruit. Manure rich in animal matter greatly increases the quantity of gluten in wheat; but it does not supply all, or more than a small part, of the gluten of which it favours the development. The greater part has to be formed from the union of atmospheric nitrogen introduced, either dissolved in water and taken in by the roots or by the leaves: probably, in most cases, by both ways. Here, then, is the very same case with that which we must contend takes place in animals. In the plant nitrogenous compounds are desirable if not requisite, in the manure, to induce the action, as we are assured by the fact that the quantity of these yielded by the plant mainly depends upon the supply of them. At the same time, by far the greater portion of the nitrogenous matter produced by vegetables must be formed within them from atmospheric sources.

In like manner, in the animal feeding solely on vegetable matter some nitrogenous principles are desirable in such food, to favour the formation of more, and to relieve the animal system of the burden of forming all the animalized principles it needs; and, as in the case of the plant, the atmosphere supplies the nitrogen requisite.

Whether this explanation be acceptable or not, whatever we may conjecture respecting its mode of action, or the reason for there being some, and not a large quantity, of such principles in vegetable food,—whether we should think, not perhaps without reason, that it accorded better with providential arrangements that the duty of developing enough of animalized principles should be divided betweeen the vital powers of vegetable and animal life; in whatever manner, in short, our fancy may lead us on this point, the fact still remains the same, that the immense proportion of non-azotized matter in such food cannot be taken in by animals merely to generate tormenting heat in tropical climates, where such food is instinctively chosen, yet where, instead of heat, the rapid renewal of the corporeal fabrics, especially in the case of such labourers as palanquin-bearers, requires nitrogenous matter in large quantity.

Throughout the preceding disquisition there are two things which I have endeavoured to avoid: the one is, any attempt, from existing experiments, to mete out all the elements which enter the body according to their exact quantity, allotting to all their destination, and distributing them in different parcels to supply the various excretions; the other is, any affirmation respecting the place of action where the elements in the blood hold commerce with each other, and with the particles of the fabric to be resolved as waste.

With respect to the first, namely, an exact estimate of quantities, interesting as such an inquiry is, it must be plain that any arguments, or hypotheses, rested upon exact amounts, must share the fate of the numerical exactness of the calculations and experiments by which such amounts are determined. If these are wrong in any degree, arguments founded on their exactness fall to the ground; but when we confine ourselves to arguments admitting of a considerable range in the quantities concerned, we rest upon much safer ground. It is for this reason I have avoided figures in a line of argument which has not required numerical exactness in the quantities referred to.

At the same time, when extensive and varied experiments conducted upon different races of men in different climates, and nourished upon various kinds of food *, shall afford safe grounds upon which to rest exact calculations, these will be highly interesting, provided care be taken not to place premature reliance on an inviting array of figures; for if they should fail us, that which is sound in our matter will be in danger of suffering loss with that which is speculative.

The other point, namely, the place where the combinations and resolutions go on in the animal system, was also a question not necessary for our arguments; it was therefore avoided. It is, nevertheless, one of great physiological importance, and forms the subject of our fourth inquiry.

* How limited as yet is our information! Who has carried out experiments on a large scale upon respiration in the arctic and tropical regions, and upon feeders on fat, flesh, and grain separately?

CHAP. V.

We have now to inquire, fourthly, in what parts of the system the vital chemistry, and therefore the development of heat, takes place; and if the capillary vessels are actively engaged in the work, and in the propulsion of the fluids circulating in them, or merely passive tubes? Thus much, at least, we are assured of, that it is in the extreme parts of the system, and not in the larger vessels, that the elements in the blood work changes upon each other. It is in or among the minute vessels, also, that the formation and solution of living parts take place.

But the question may be asked, does the change take place within vessels, or on solids or fluids exterior to them? It will help us much, in judging upon this question, to consider first the power which determines all the actions. Now these actions may be considered as of two kinds: one, all changes going on in the elements in the blood, as between those of the chyle and the oxygen and nitrogen brought in from the lungs; and the other, the deposition and absorption of living particles. What is the power which determines the kind of action which shall take place, and the extent to which it shall go on? Unquestionably the vital. There is a living chemistry, an operation regulated entirely by the vital power, which can unite or separate at will the same elements, —which can deposit or remove substances, form them out of their elements, and tear them asunder when formed. It is no doubt true that the effort of the vital powers is aided or retarded by the nature of the materials offered to it. Its work is rendered easy by having the compounds it needs offered to it ready formed. Hence flesh diet calls for less effort, where flesh is wanted; but we have seen abundant reason for the conviction that not only from ready-formed principles, but from dissimilar, the body may be nourished, if nature be but supplied with the elements wanting to transform them into animal matter. The presence of some proportion of principles similar to those to be formed greatly aids the work. Hence, in the case of animals, even vegetable food contains a proportion of nitrogenous principles; and in the case of vegetables, which have to produce for the nourishment of their stems or fruit substances containing nitrogen, we find the soil must offer them a little of these, as it were, to start the work; yet we know that it is but little of all that is formed in the plant.

Viewing, then, the vital power as the main controlling agent, can we imagine it to act without any instrumental means? How is it possible it should separate particles from each other, shift their places, unite them to others — sometimes in binary, sometimes in ternary, at other times in quaternary compounds, — unless by some material instrumentality?

Now, where particles in a soft or fluid menstruum are the subjects to be acted upon, it is difficult to propose any other tools than tubes, — first, of different calibers; secondly, each able to vary its own caliber, sometimes in one spot, and sometimes in another;

thirdly, the orifices of such tubes; fourthly, their lateral pores. Such a machinery as this we find every where: without it, it is not easy to imagine that the union of elements could be as entirely subservient to the vital power as it is. Without such a machinery, it might be safely affirmed there is no such thing as the formation of substances in the body. It cannot be shown that any such thing exists as extra-vascular vital action.

That the minute vessels of the body are actively concerned in working the changes which take place in the fluids within them, we have strong proof. To them alone can we trace the duty of determining the composition and form of the different structures of the body. It is while circulating through the minute vessels, that the blood undergoes its change from the arterial to the venous state. The reason appears to be, that in the capillaries the particles of the blood are all brought into contact with the walls of the tube, and thus under the influence of the extremities of nerves so largely distributed in the extreme parts of the system, and of the lateral pores and orifices of the capillaries.

The question here offers itself, which has been so much discussed by physiologists, are the capillary vessels merely passive carrying tubes of the blood, or are they actively engaged in the propulsion of their contents? A review of all that has been, or may be said on either side of this question, would occupy much too large a space. I shall here offer only a few remarks upon it. The arguments in favour of the inactivity of the capillaries, and the evidence of the experiments adduced to establish it, appear to me far from satisfactory. The fact of a *local* distension of the vessels has never been properly explained upon the passive hypothesis. It must, on all hands, be admitted that some cause besides the action of the heart, which is *general*, must be found for *local* action,—for the determination of blood to particular parts. It is said that the vessels of such parts are more relaxed than others, and yield more to the pressure of the blood propelled by the heart.

When we look upon the flushed countenance of a choleric person, it is difficult to associate with his *excited* state the idea of *passive relaxation*. But the fact assumed is itself an admission of previous action. There can be no relaxation of a living part where there was not previous contractile action. It is an admission that the coats of the vessels resisted the pressure of the blood by their contractile power; for mere elasticity, like that of inactive inanimate matter, admits of no relaxations and contractions. The question then, after all, is one only of degree. The one party contends that the vessels contract enough upon their contents to resist distension beyond some supposed limit; the other party holds that the contraction of the capillary vessels is carried further, and that it is instrumental in propelling the fluids within them. The common objection to this, that contraction ought to prevent the entry of the fluid, instead of favouring its progress, will have no weight whatever, if we consider the contraction to be of a vermicular kind; nor will it do to object, that such contraction is not observable. Where the vessels are so extremely minute, a peristaltic action quite enough to propel the contents would be altogether imperceptible. The smallest progressive contractions of a tube, we may be assured from the nature of incompressible fluids, will greatly further their progress along them. This fact is seen in the sucking of a leech, and its gradual distension with blood. How small are the contractions which travel along the body of the animal, when compared with its thickness, and with the caliber of its many-plied stomach! Let us suppose the contractions of the capillary to bear the same proportion to its caliber, and they will elude all our powers of artificial vision.

It is a fact worthy of notice, that when the capillaries are irritated, and their contractions become discernible, they have been observed to be of a vermicular character. This is mentioned by Doctor Hastings and others, in their experiments.

The principles of hydraulics, when applied rightly to this question, make the supposition of the entire circulation of the blood through the infinitely subdivided capillaries being effected solely by the heart's action, one painful beyond measure to the understanding. Could we believe the heart capable of of exercising the prodigious force requisite, such an arrangement is opposed to all the evidence we have of the beautiful economy of means pervading the works of nature; for this, at least, we know, that an infinitely smaller power in the capillaries would effect the propulsion with a certainty we cannot conceive the former to possess. Were the most trifling obstruction to take place in the capillaries, it never could be cleared away by the action of the heart, had it the power of all the muscles of the body. Not to pursue the subject to a greater length than my space permits, I shall instance a familiar, yet very remarkable phenomenon, which appears inexplicable, unless upon the ground that the capillary vessels are actively concerned in the propulsion of their contents. I have already stated the argument, in considering the effect of vapour upon the lungs, in papers on artificial climates lately published in the "London Medical Gazette," and may now transfer it in nearly the same words.*

"It is not the vulgar only, who, viewing commonplace facts as matters of course, fail to have their attention arrested by them. Powers of observation superior to theirs do not always guard us against this influence of familiarity to induce neglect. Thus it is common to describe the effects of warm water, either when acting on a healthy member, or when soothing an inflamed part, as a 'relaxation occasioned by the warmth and moisture;' but this is to cover an obscurity with an expression equally obscure and undefined. Many physiologists of note have attributed the phenomena of inflammatory *distension* to relaxation in the capillary vessels of the affected part; and here we find the same term 'relaxation' applied to that condition of a part, the *opposite to distension*, which is produced by a long-continued application of moisture. Were a person unacquainted with therapeutics, having witnessed with astonishment that which would indeed be surprising, were we not familiar with it, namely, the action of a fomentation or poultice on an inflamed part, causing a shrivelling of the surface and diminu-

* Vol. i. p. 968. March, 1842.

tion of the swelling, to ask his surgeon the cause of it, I am not aware that any satisfactory explanation has been propounded which could be offered, or that any can be, until some decided opinion is established upon the circulation of the blood in the capillary system of vessels.

" In a still more familiar case, and one not complicated with any question involving in it the nature of inflammatory action, namely, the very vulgar but very curious phenomenon of the well-soaked hand of a washerwoman (or still better of a hand well soaked for the first time), were she to hold it out to a physiologist, and ask him why the water which swelled all other things left in it produced an opposite effect upon her fingers, causing them to become so bloodless and shrivelled as actually to measure less than usual, I know of no received explanation of this fact which would afford him a ready, and at the same time a satisfactory, reply.

" When the hand has been long immersed in water, especially if the epidermis is scoured by alkali or soap, the appearance which presents itself, being a matter of course in the sight of the vulgar, is not likely to lead to such an inquiry as has been imagined. Nevertheless, it could not be explained by any laws yet established with respect to the physiology of the skin and of the circulation. The action is by no means confined to the epidermis, as some have thought. Were it so, indeed, there would result from the thickening which takes place in it, as in all other organic matters not possessed of vitality, when thoroughly soaked, an increase in the bulk of the fingers; whereas, when the experiment is favourably made, the diminution is very considerable. The wrinkling, translucency, and shrunken appearance of the fingers, show that they are in a bloodless state; that an effect has been produced, not upon the colourless only, but even upon the red-blood circulation, which is not travelling out to its usual limits. Confined, in the instance before us, to a small portion of the surface of the body, the effect of this disturbance of the circulation is not perceptible; but were so considerable a recession of the blood to become general, the consequences could not fail to be serious. In cases of cholera in India, when running a rapid course, I have often been struck with the similarity of the extremities in their shrivelled and bloodless appearance to a member long immersed in water; a state, however, which I conceive to be an effect, and by no means a cause, in this disease.

"The question, then, before us assumes two forms. First, the simpler case, in which a marked effect is produced upon the surface of a healthy member by a long immersion in water; secondly, the more complicated case, where an important, though less considerable effect, is produced by the same agency upon the surface of an inflamed part.

"In both these cases we find, at the threshold of our inquiry, certain physiological points which must form the very ground-work of our reasoning, to be still subjects of controversy. These are, first, whether or not the skin can absorb a bland fluid like water applied to it; secondly, whether the capillary vessels of the circulating system are actively engaged in propelling the blood, or are mere passive hydraulic tubes allowing it to flow through them by the propulsion communicated to it by the heart; thirdly, supposing the capillaries are active themselves, whether the blood is the natural stimulus which excites them, as it does the heart, to action; and whether, as in the case of this organ, the action varies with the degree of its stimulating quality.

"These are points to be decided before we can reason upon either of the cases proposed. In the latter case, where the liquid acts upon an inflamed part, another much controverted point has to be settled; namely, whether in inflammation the capillary vessels passively endure the great distension through relaxation or debility; or whether this distension is compatible with an increase of activity in them, or is even caused by it. Presuming my readers to be acquainted with the position to which controversialists have brought these questions up to the present time, I shall not occupy their attention by a discussion of each at length; but I think the interesting and important cases before us may, by an argument conducted in the following manner, not only receive whatever explanation they admit of in the present state of our knowledge, but may even reflect light upon the physiological points themselves. Commencing, then, with the simpler case of a part not inflamed, let us suppose that the effect of long immersion in water had never been observed, and that the question were proposed, as to what effects were likely to result from it, to two physiologists — the one of what may be named the passive or negative school, the other of the active or affirmative school.

" The former would argue thus : — ' More influenced by the views and experiments of those who deny any appreciable power of absorption to exist in the skin, I conceive that no decided effect could be produced by a bland fluid like water acting upon a small portion of the body. But granting that the fluid would be absorbed, since I deny (he would say) to the capillaries any active contractile power or concern in the circulation, but, with Müller and others, consider them mere passive tubes conveying the blood propelled by the heart, it follows plainly, if water were absorbed at all freely, so as to enter the extreme vessels of the general circulation, partly at their orifices, and in part by penetrating the tissue forming their coat, as is maintained by many, that its bulk must be added to that of the fluids in the part, and a distension of the part would of necessity result. If it were the hand, all the fingers would swell in proportion to the absorption, and to the time requisite for the fluid thus entering to be pushed on with the blood by the *vis a tergo* originating in the heart. Therefore I conclude that the immersion of a member in water would either produce no effect upon it, or if the fluid really entered, it would become swollen in proportion, so long as the absorption into the general circulation continued ; and it would remain swollen for some time after its removal from the fluid, until the vessels of the part were gradually relieved of the load.

" ' With respect to the second case, where the part to be subjected to the aqueous experiment is inflamed, upon the above grounds I could expect (he would say) no good from humid applications. In addition to the reason just stated why the fluid should increase the swelling, I find a cause which would have the same effect in the relaxation of the capillaries, which, in common with high authorities, I hold to exist in all cases of inflammatory distension, and to be a passive cause of it by allowing the heart to inject more blood into the vessels. If, in inflammatory action, the capillary vessels of the part are in a state of *passive* distension, I may surely anticipate the aggravation of this condition by the absorption of water into these vessels, if indeed fluids can enter the system by the skin.' It appears to me impossible to object to the arguments of such a physiologist, provided we could grant his premises. His anticipations in both cases, of water acting upon a healthy and inflamed part, flow naturally and inevitably from his physiological views.

"On the other hand, as a physiologist of the active school, I would myself argue thus : — 'I consider the skin, like other membranes of the mucous character, to possess the power of absorbing congenial fluids freely, as soon as they have found their way through the epidermis. Tardily as uncongenial fluids are taken up by the skin, such as poisonous solutions, which I grant would appear from the experiments of Seguin, Rousseau, and others (and it were no cause for surprise that the skin was endowed with some power of refusing such things), we have in the experiments of Drs. Edwards, Southwood Smith, and others, evidence from which we may infer a considerable power of absorbing pure water. All that appears to me necessary is the soaking of the epidermis. In its usual condition it would seem, like any other fibrous matter the texture of which is oiled, to be nearly water-proof; that is, each of its minute scales may be considered so, but overlying each other, they appear to rise to give vent to fluids exhaled, but to be nearly closed to fluids passing from without. By sufficient soaking, however, we may view the whole texture of the scales themselves as rendered permeable by water, which, on the oil being removed, enters readily into their substance. The epidermis must then act, as any other inanimate porous matter, by the physical property of capillary attraction absorbing water at its outer surface, and, like a sponge, conveying it to the true skin within.

"'When fluids have reached the true skin, experience, as well as the analogy of other membranes of the mucous character, shows that a ready absorption takes place, partly through the vital activity of the absorbent orifices of the vessels, and in part by that kind of permeation which has been named endosmose, and which appears to arise from a capillary or interstitial adhesive attraction of an elective character.

"'In this manner a fluid may, and it is proved does, enter the substance of the true skin, and take up a position in the capillary vessels; and when there, I would expect an effect the very opposite to that which you have described. Instead of a swelling of the part, which I grant must result from the presence of any extra quantity of fluid, provided the vessels are merely passive carriers of the contained fluids, and not roused to action by the presence or quality of them, — instead of this distended state I would expect a diminution of the fulness, perhaps to the extent of a shrivelled and even bloodless state of the part, and for the following reasons :—With many others I entertain the opinion, that the evidence tending to prove the circulation of the blood in the extreme vessels to be maintained chiefly by an active contractile power of the capillaries themselves, vastly preponderates, both in amount and force, over that in favour of a passive state of the vessels. Having afforded all that attention to the facts and arguments adduced against an active virtue in the capillaries, which deference to the authority of Müller and others should command, I am unable to discover in the whole of them evidence at all conclusive, or to be compared with that which assigns an active duty to the smaller vessels.

" ' Again, we can hardly doubt, the contractility of the capillaries being granted, that the stimulus which excites and modifies their action is afforded by the blood, since we know it to be the agent which urges the heart to action, and that the movements of this organ are regulated in a great measure by the quality of the blood. We know, also, that the blood is the stimulus which gives action to the cerebral powers, and, indeed, that every hollow organ in the body is affected by the fluid it contains. Doubtless, then, as the blood varies in quality, will the action of the capillaries vary. If we render it irritating, as by the presence of alcohol, the extreme vessels will be excited, and a temporary flushing or distension will result; and, on the other hand, if we dilute it, they will, wanting the usual stimulus, flag in their action. This diminution of the main power by which the blood, according to my premises, is forced onwards against the obstructive resistance offered by the multiplied subdivision of the tubes, must be followed by its natural consequence—namely, that the red blood will not travel out to the limits to which it is usually conveyed; much more of it will return by the larger anastomosing branches, and the capillary system will be comparatively empty. Now, I imagine that the presence of a bland fluid like water, by diluting the blood in the smaller vessels, cannot fail of producing these effects.

" 'I would be led to expect the absorbed water to be more efficient, acting in this manner, in diminishing the fulness of a part, than by its mere bulk in distending it; for it is probable that a very little water absorbed into the capillaries would suffice for lowering their tone for the time. As to the epidermis, it would, of necessity, swell like any other distensible porous matter not possessed of vitality; yet the thickening of it might not, by any means, compensate for the flaccidity of the vascular parts beneath, and upon the whole a perceptible shrinking of the part might be expected. Thus, while all inanimate organic matter swells by immersion in water, I would anticipate, in the case of a living member, a a shrinking and empty state of it surface. In proportion as means were employed, by oiling, or otherwise guarding the epidermis against the action of the water, we might expect the effect of the immersion to be small; and, on the other hand, if by the aid of soap and alkali the natural unctuous matter were removed from it pores, the effect ought to be proportionally considerable. The hands of females employed in washing ought, therefore, to present a shrivelled bloodless appearance; and this we find to be actually and invariably the case.

"'Again, in the second case proposed, namely, the probable effect of humid application to an inflamed surface, upon all the grounds stated I would anticipate, not injury from increased distension, but benefit from a lessening of it. Viewing the capillaries as actively concerned in maintaining the ordinary circulation, I hold the opinion, in common with many of the most successful inquirers, that these vessels have their activity much increased in a part involved in inflammation. Considering, also, the blood to be the natural stimulant which excites them, I conceive an undue excitability to exist in them when inflamed, which causes healthy blood to be then too coercive, and blood in a febrile state still more so. Under these circumstances, a continued absorption of water, as a diluent of the blood on the spot, can hardly fail of soothing the vessels, by lowering its stimulus, and thus preventing their distending themselves by undue action; so that, just as in a natural state of a part, when a proper correspondence exists between the quality of the blood and the sensibility of the vessels, if we cause a considerable absorption of water we shall disturb this condition, and render the blood too feebly stimulating, and the part, as a consequence, too little injected with it: in the other case of a morbid irritability of the vessels, where the blood is too stimulating, we may, by diluting it on the spot, bring about a better correspondence between its quality and this state of the vessels, and may cause the too great injection of the part to subside nearly to the natural condition of fulness. Hence benefit, not injury, should arise from moist applications to inflamed parts.

" ' This again we find to be the case, and it confirms in a striking manner the view here taken. There is one point, however, which it will not do to overlook. While all other objections to the theory of activity in the capillaries have already been refuted with great ability, especially in the admirable work of Dr. Bostock, one remains, which, though nearly set aside, is not altogether cleared away. It is contended by many able objectors, that contraction of the parietes of a tube involves with it, as a necessity, a diminution of its caliber, and therefore of the bulk of a part made up of such vessels. Hence that we can hardly understand how activity in the capillaries could favour the entry of the blood into them; and still less how an increase of this contractile activity could conduce to, or even be compatible with, the state of inflammatory distension. This objection, and this alone, appears to call for attention. This objection, however, is apparent, not real. It exists only in the terms in which it is couched, and therefore in the minds of the objectors, and not necessarily in the statics of the case. If any hollow tubular body were to contract upon its contents in every part of its entire length simultaneously, it is undoubtedly plain that the tendency would be to drive them out of it at both ends. Should some *vis a tergo* prevent the contents from going in one direction, the propulsion certainly would take place onwards in the opposite. As the vessel would become filled again at each relaxation, which always follows contractile action, and the contents be propelled at each contraction, some of the phenomena of the capillary circulation might be thus explained. But such a movement does not accord with the perfection of natural provisions. It is manifest that the reaction against the *vis a tergo* would, to such an extent, be an opposing burden on the action of the heart. Moreover, it would by no means serve even as an awkward explanation of the phenomena of inflammatory distension. Hence, we cannot but feel the force of the objections against the possibility of capillary activity occurring in conjunction with inflammatory distension, pressed by Drs. Thompson, Wilson, Philip, Hastings, and others, until we are prepared to abjure the notion of the contraction being of the nature described, namely, simultaneous throughout the length of the vessels. But if we substitute, in our views, a vermicular or peristaltic contraction for the continuous, we may, I think, remove the difficulty at once. It is by such a *progressive contraction* from the mouth downwards that a leech *distends* itself with blood. In a healthy state, the periods of this peristaltic action of the vessels may harmonise with the action of the heart. The action may be viewed as commencing indeed, *though very feebly*, in the larger vessels, and following up each systolic wave of the fluid, until in the capillaries it is exerted with such force as to be the chief propelling power. In health, then, when all the actions are moderate, at a certain point of the gradual diminution of the vessels by subdivision, the proper coloured portion of the blood being refused further entrance, as the vessels grow too small for it, will naturally find its way through larger vessels into the venous circulation, while much of the thin colourless portion of the blood will pass on through the numerous minute tubes which refused the former, and these may constitute the system of the ordinary colourless or extreme circulation. But when such a vermicular contraction is increased, it is obvious the effect must be an increased propulsion of blood along the vessels, and it is only necessary to suppose this propulsion to exceed the ability of the vessels branching from them to the veins to carry off the red blood, when, as a natural consequence, it will be driven by each successive contraction along tubes ordinarily too small to receive it. There is no difficulty in supposing the parts of the tube which are exerting contraction to exceed in force the mere elasticity of the quiescent parts. We have a familiar instance of this in the intestinal canal, which often becomes distended by substances carried on entirely by its own peristaltic action. Such a state being granted, distension may be its consequence. The case of a leech, already noticed, is an apt illustration of this kind of action. As the distension proceeds, the tube at last cannot contract upon its contents, the fluid within being incompressible. The case of the leech only differs from that of the capillary in the detention of the blood being absolute in the many-celled stomach of the animal, while in the vessel it does pass on, but too slowly to clear the way. The two cases, as to the causes of the distension, run sufficiently parallel ; their difference is only in degree.

" 'Thus in the case of the capillaries of an inflamed part, increase of contractile action in them, so far from being irreconcileable with the distended condition, is the very cause which should produce it, if the contraction proceed with vermicular progression. Such a peristaltic propulsion of the blood would well account for the throbbing felt in an inflamed part, and for the undulatory motion of the blood observed to extend even to the venous circulation. If the action of the heart might be conceived to account for the former, I cannot accede to the opinion that it could possibly extend itself through the infinitely divided capillary system, so as to be perceptible in the venous circulation; whereas the progressive contraction assumed here in the capillary system, and supposed to act in concert with the heart, could well explain a perceptible amount of undulation being thereby maintained in the blood even as far as the veins. Thus, a fact instanced by some as incompatible with activity in the capillaries, becomes one among the many which receive a better explanation by supposing these vessels concerned in the propulsion of their contents, provided always their action be vermicular, than by the passive hypothesis.

" 'It would appear a general rule, that wherever living tubes are concerned in the propulsion of their contents, their contraction is of the vermicular kind. From this we might infer, by analogy, that if the capillaries are active, their action must be peristaltic; and it is a fact worthy of note that physiological experimentalists, in whose observations the highest reliance may be placed, both on account of the character of their authority, and because they are not advocates of the doctrine of activity, and mention the fact merely as an observed truth—it is worthy of note that in many of their details of experiments on the skin of animals, the capillaries of which had been excited by irritants, until their action became discernible by the sight, the motion was observed to be "vermicular," "oscillating," and "peristaltic."*

"'That Dr. Thompson did not find such motions, in the healthy state†, in the web of a frog, is no matter of surprise, when we consider that many of the vessels of the web approach in their comparatively large size, and may therefore in their structure, to arteries, the small contractility of which is admitted on all hands; and that others should not, in ordinary states of parts experimented on, discern a peristaltic action, is perhaps no other than what might be expected, since even in so large a cylinder as the body of a worm or a leech, a very slight depression traces the course of a contraction sufficient for the propulsion. In a minute capillary, then, we may almost be surprised that, even under great excitation, the peristole of the vessel could be rendered visible.'

"On reviewing the positions of either disputant it would, I think, be difficult, granting our physiological data, to refuse our inferences. With respect to the one, if the skin does not absorb even a congenial fluid, as is contended by many even to this day, humid applications to the surface should produce little or no effect; and if the power of absorption be granted to the skin, but a power of propelling

* Dr. Hastings on the Lungs and Mucous Membrane, page 27, 28. 85. and others.

† Lectures on Inflammation, p. 77.

the blood be denied in its vessels, then the effect of humidity would be to cause a temporary swelling of the part proportional to the quantity of water absorbed.

" Respecting the views I have adopted on the side of capillary activity, if we grant to the skin absorption, and to its vessels excitability by the stimulant quality of the blood, as the means by which they bring on the blood and fill themselves with it, we must also grant that the withdrawal of this stimulus ought to cause a subsidence of their action, and a consequent partial emptiness of them ; and hence a flaccid condition of a part made up of such vessels. Lastly, we ought, upon all these grounds, to expect this effect to be favoured by the removal of oily matter preoccupying the pores of the inanimate epidermis. What, then, are the experimental facts ? Wholly at variance with the passive doctrine, and in strict accordance with the active. Moisture, in whatever form it is applied, if its temperature be not irritating, invariably tends to moderate the distension of an inflamed and to render flaccid a healthy part. In the latter case, a simple and familiar act, performed every day in every cottage of a washerwoman, seems an *experimentum crucis*, a guide-post pointing us away from the mazes of capillary inactivity to views of tone and action in the extreme vessels, discernible alike in this case as in that of secretion and excretion."

However commonplace may be the shrivelling action of a poultice or of a washing tub, it would be an act of a vulgar mind on that account to refuse attention to it.

The effect is a phenomenon which the physiologist who questions capillary activity may be challenged to explain. It is an experiment upon a part in its natural, undisturbed state, and therefore incomparably more valuable than so many advanced in physiology, where conclusions are drawn from experiments which, from their torturing nature, disturb the natural condition, and thereby vitiate the conclusions.

The great importance of moist applications, and of unguents, which act by retaining the moisture on the surface of the part, ought to command our attention to the *rationale* of their action; and, as a fact, this observed action serves as a strong position for the advocates of capillary activity, upon which alone they may take their stand: until it is carried, they might leave other supposed objections at rest. The very terms employed to define the use of a fomentation or poultice place the passive hypothesis in a contradictory position. Every one admits that the action of a fomentation or poultice is to produce relaxation of the vessels of the part. It is not for a moment believed that it braces them up. But relaxation, according to the passive hypothesis, is attended with *distension* of the vessels, for it is employed to account for inflammatory distension. Then should the relaxation caused by a fomentation or poultice be followed by a distension of the vessels. Humidity acting long on a surface ought to increase its distension with blood. If the painfully distended state of an inflamed part resulted from relaxation and debility, how aggravating of the distension and suffering ought to be the effect of a poultice, or fomentation! whereas, if the right temperature is given to the fluid, it is invariably soothing, and just in proportion as its *relaxing* effect is complete does it cause a *diminution of the distension* of the vessels of the part.

Again, the case instanced as the strongest proof in favour of the passive doctrine of inflammation will be also found, when properly scrutinised, actually to militate against it. It is contended that what is termed passive inflammation obviously results from a relaxation of the capillaries from debility. Thus in fevers of debility, especially in old persons, the mere gravitation of blood towards the lowest part of the lungs will sometimes produce a pneumonia.

But how is it that this effect is confined to certain kinds of debility only, and that it is not a general or even common effect in all others; that it does not take place, indeed, under circumstances of the most extreme debility, which are not accompanied by febrile or inflammatory irritability? It is because this irritability must be present to complete the effect. What, then is its nature? It is readily explicable upon the doctrine of capillary activity, and in complete accordance with it.

While we contend that, under ordinary circumstances, the motion of the fluids in the capillaries does and must depend mainly upon their own contractile action, and that in inflammation in general the distension is produced entirely by their increased excitement, the obvious fact is not for a moment questioned, that any cause pressing the fluids towards the capillaries will favour this distension. In proportion as they have most tone, will the distension be mainly their own work, and any *vis a tergo* will have less influence; but as they are weaker, so will any other cause, as gravitation, have a larger share in the work. But gravitation alone will not cause inflammatory distension; for, in cases of the most extreme debility, it ordinarily does not. It only presses the fluids towards the capillaries, and at the same time tends to retard their return in the veins. When the capillaries are in a state of febrile irritability, it stimulates them to receive, and, by their own vermicular action, to gorge themselves with it; and no doubt, when distended, in proportion as the system is debilitated they will be less able to unload themselves. We must bear in mind that debility is quite compatible with increase of action; we perceive it, indeed, in the case of the heart. Its struggles for action are often increased by debility. We may with advantage here again appeal to the case of the leech. There can be no doubt that if the lower end of a vertical tube, filled with a liquid, were introduced into the mouth of a leech, in proportion as the animal was weak, so would the pressure weary it more than when vigorous, and would cause it to receive, and propel on, the fluid, until it was full. Yet no one doubts of the distension of a leech being an active work on the part of the animal. We may often see the leech so distend itself by its own action, that at last it cannot contract upon its contents. This is an effect of vermicular action common in living tubes. How often do portions of the alimentary canal become so loaded, as to be unable to contract upon, and propel the collections within them! Yet we know that the movement of their contents is entirely due to a peristaltic action. The action of the abdominal muscles cannot be, in the smallest degree, propellent of the intestinal contents, excepting upon such as are in the lowest part of the rectum. Upon the contents of all higher parts of the intestinal tube their action is altogether secondary. By pressing the canal upon its contents, they excite to greater activity the peristaltic movement; and it is the latter which carries on the matter within. If there is not a regular consent between the action of a higher and lower portion, we know, as before remarked, that the distension of a part often takes place, and is self-produced, as in the case of the leech, until, when it is extreme, the canal is so much stretched as to act more feebly than before. Yet we do not doubt but that the progress of the contents is altogether due to intestinal activity, not passivity.

Between the active and passive hypotheses, which latter, as already remarked, in its very definition involves a usual state of tone, or, in other words, contractility, the comparison has been carried far enough to establish the much greater credibility of the former, and as far as our present space admits of; though the whole argument might alone fill a volume.

PART III.

ON THE DISSIPATION OF ANIMAL HEAT, AND ITS INFLUENCE IN PRODUCING LOCAL DETERMINATIONS OF BLOOD, ESPECIALLY TO THE HEAD.

CHAPTER I.

ON THE DISSIPATION OF HEAT.

WHILE the production of animal heat has been a subject of much inquiry, the dissipation of it has certainly not received adequate attention.

In the preceding pages the strongest proof has been adduced to show that the power of cutaneous exhalation, which is entirely dependant on the hygrometric state of the air around the body, would, in numerous instances, be quite unequal to the task of carrying off the animal heat in a tropical climate, if the production of heat bore the same proportion to food, even of the same quality, that it does in colder climates. There cannot be, I conceive, a doubt that it is a prerogative of the vital power so to determine the manner in which the compounds shall be formed which are to be daily discharged, as that the heat generated in these combinations shall not exceed the quantity which can be carried off by the channels of outlet for it; namely, the surfaces of conduction and evaporation. There can be no doubt that the hygrometric state of the air often existing in a tropical climate has been overlooked by physiologists; and that the great power of perspiratory evaporation in removing animal heat, under favourable circumstances, has led to the too hasty conclusion that it is always a sufficient cause, whenever the temperature of the air is too high for the animal heat to pass off by conduction, or rather by that form of it named convection.

At the same time, there can be no question but that, under ordinary circumstances, in all climates, a large quantity of animal heat is carried off by the various channels, especially in cold climates. Now this may be divided into two portions,—that which passes off by the lungs, and that which is discharged by the skin.

With respect to the first, there are one or two interesting points which it is proper to touch upon. We are wont to consider respiration a heating process, and in this we are doubtless correct; we are right, also, in the opinion that the heat produced is, for the most part, proportional to the freedom of the respiration,—that is, that the oxygen introduced in respiration must increase with the increase of the elements which have to be discharged from the lungs; but we greatly err if we think, because more air must be had to produce more heat, that therefore more air respired must always produce more heat.* The system must have more air when it requires more to increase the heat-developing combinations, since it cannot increase these without more oxygen; but it by no means follows that the presence of more

* Appendix VII.

oxygen will force it to increase the combinations; and, if we are duly observant, we shall find abundant proof that the quantity of the already respired oxygen which is *employed*, is altogether determined by the wants of the system, and is under the control of the vital powers.

Hence, if we should increase the frequency and depth of our respirations, at a time when no increase is called for to carry off more matters from the system, either derived from the fabrics of the body or from the food, there will not be any consequent increase of heat. No more heat will be developed, nor any more oxygen employed, so far as our observations permit us to decide, than previously; but, on the contrary, an opposite effect will be produced. The additional air introduced into the chest will have a cooling effect. I have observed the natives of a hot climate to breathe much more through the mouth, and to heave the chest more in respiration, than we do in England; and I am conscious in my own person of a difference. Indeed, with the panting for breath in a very hot atmosphere every person who has dwelt in the tropics is but too familiar. It is an instinctive effort to rid the system of superfluous heat, and not, of course, to form more heat; and free breathing conduces much to relief. Hence, I have observed ladies to be willing enough in hot weather in India to desist from tight lacing, for the purpose, according to their own statement, of breathing more freely. In such a climate, the freer respiration acts chiefly, no doubt, by increasing the evaporation from the lungs. Indeed, I have observed it in an atmosphere above the temperature of the body; but when the temperature was very much higher, though the wind at the time was intensely arid, and therefore all the fitter for cooling by evaporation, I have often felt an instinctive caution not to admit an undue quantity of so very hot air, but to keep the mouth shut, and respire through the nostrils. This has been the case in the mid-day air of the hot winds, the thermometer being between 110° and 120°.* It appeared to me that the vapour the lungs could afford, was not enough to counterbalance, by its evaporation, such an introduction of heat; for, with the air between 85° and 95° or 100°, I have often observed, as already mentioned, in my own case and that of others, a more copious respiration than in colder weather.

All these facts are of value, since they point us away from erroneous conclusions, to which the strong love of symmetry in our theories might entice us, and open to our view, in conjunction with others already fully set forth, the various and surprising ways in which the same apparent means are made by Providence to produce different effects under different circumstances.

The subject now leads us, in considering the dissipation of heat by the skin, to questions of the highest interest and importance; both on account of their novelty, and from their connexion with a very fatal class of diseases — those arising from determinations of blood to the head.

* The highest I ever noticed the thermometer at in India, well shaded on the north side of a house, and properly placed, was 122°.

With a view to brevity, I shall not only avoid any detailed accounts of the cooling action of the skin by conduction and evaporation, but I also omit several observations made upon these functions of the surface of the body under peculiar circumstances, which were interesting and instructive. Some of these related to the effect of diminished atmospheric pressure, and others to high temperatures, and great hygrometric changes.

I may mention one fact, — curious, as giving us a direct view of the limit of cooling by evaporation from the skin, and useful, as offering to the tropical practitioner a hint respecting the clothing he should recommend to Europeans according to peculiarities of constitution, and the *temperament** of the atmosphere. In the case of the intensely hot and arid climate of the provinces of India west of Patna, and especially of those west of Benares, the only persons I have met with who ever wore any flannel next the skin, during the season named the hot winds, were a few of those whose perspiration was profuse, and who feared, or were liable to, a sudden suppression of it. They do well to wear it; but I have no doubt, where frequent exposure to the hot wind is unavoidable, that persons with a dry skin will find most benefit from the use of flannel. Such was my own case; and few were more exposed, in the performance both of public duties and experimental inquiries.

Finding the surface of the skin in the hot wind to rise considerably above the temperature of the blood,

* *i.e.* The properties of its hygrometric and thermometric state combined.

and to be at the same time parched, I clad myself from the neck to the wrist and ankles in stout flannel. The immediate effect was a moist condition of the skin wherever so covered, and a fall of its temperature of from five to ten degrees, and at times even more. If I withdrew the flannel from a part, leaving over it only the usual thin cotton dress, upon remaining some time in the outer air it became of a febrile heat, while all the flannel-clad portions were actually cold to touch, and humid, showing that the heat of the uncovered limb was not really febrile, but a conduction inwards of heat, which there was not perspiration enough to carry off; the barrier of perspiration which the skin sets up, having been, as it were, passed. The same results were produced day after day, and season after season; and I cannot doubt that by enveloping myself thus in flannel in the hottest weather, and at the same time carefully avoiding the direct rays of the sun, I was enabled, with a constitution ill suited to the climate, not only to remain in it many years, but to endure an unusual amount of exposure to the air. It is obvious, indeed, that the condition must be dangerous when the cutaneous barrier has been carried by the assaults of outward heat.

It was a curious point, worthy of remark, that not only was the outer heat kept off, but the skin fell considerably below the temperature of the blood.

The porosity of the flannel, on the one hand, aided by the great dryness of the air without, sufficiently transmitted vapour, carrying away latent in it, animal heat; while the slowly conducting property of the flannel, on the other hand, sufficiently checked the entry of *atmospheric* heat to cause the exit of heat to exceed the entry of it, and this with a smaller expenditure of perspiration than the skin could afford; hence its durably cool and humid state. This I conceive to be the philosophical explanation of the simple but curious fact.

CHAP. II.

ON THE LOCAL DISSIPATION OF ANIMAL HEAT, AND ITS CONNEXION WITH LOCAL DETERMINATIONS OF BLOOD.

This is at once a very curious and important subject.

The consideration of it will lead us on to conclusions, in the next chapter, so opposed to long-established and popular notions, that there may appear some temerity in broaching them.

Nevertheless, if every step of the argument is taken with logical correctness, and the resulting demonstration shall be also fully supported by facts, we need not hesitate to produce our facts and arguments, and to invite controversy upon a point not only of scientific interest, but involving also questions of the highest practical importance in medicine.

In the first place, it is obvious, that while the production of animal heat is governed by the vital powers entirely, the control of the vital powers over the dissipation of it is very limited. Their control is confined to the exhalation of vapour from the lungs and skin, the amount of which is doubtless as much under the influence of internal vital, as of external physical powers. Hence, under the same outward circumstances, different persons will be found to exhale very different quantities of vapour, and of consequence to be differently affected by the same temperament of atmosphere.

But while the quantity of the fluids eliminated is determined in a great measure by internal causes, or, in other words, by peculiarities of the individual constitution, the quantity of them which is evaporated, after they are eliminated, is plainly altogether beyond the control of the vital powers. It is governed entirely by the *temperament* of the air around, and by the nature of the clothing. Moreover, the quantity of fluid which is exhaled will be materially modified by the readiness with which these outward influences, of atmosphere and clothing, permit it to pass off.

Next, with respect to the dissipation of heat by conduction. Over this, it is obvious, the vital powers can have no control, excepting by changing the conducting quality of the surface, as by varying the coat of wool or hair on the animal. In the case of man, then, we may consider the dissipation of heat by conduction entirely governed by external causes, the temperament of the air, and the character of the clothing.

In the next place, we know that it is incompatible with *life*, that the body should, even for a short period, vary beyond a very few degrees from the standard ordained for it by the Creator, and with *health*, that it should for any durable period vary at all from that temperature. This, in the whole human race, appears to be confined within the small limits of four degrees, that is, between 96° and 100° of Fahrenheit. That this perfect equability should be preserved, it is plain, in the case of any individual, the heat dissipated by the various outlets together must exactly correspond with the whole quantity generated; for if it were greater or less, the body would fall or rise in temperature.

But the quantity of heat produced by different individuals by no means corresponds with the extent of the surfaces discharging it from the body; hence in one person much more may, and must, pass off, from a given surface of the lungs or skin, than in another. When in the same atmosphere, then, the one will require much more clothing than the other. Hence the obvious propriety of not varying the amount of a person's clothing, provided he feels at the time neither hot nor cold, until we are certified whether he will learn to produce more or less heat, according as we decrease or increase its amount. Now whether he will do this will depend, not only upon the quantity of food he takes, but upon the manner in which his system will manage that food; for, as established in an early chapter, there cannot be a question that the vital power can, and often does, from the same elements, so rule the combinations as to produce very different quantities of heat.

Let us now attend to *partial* abstractions of heat; that is, abstractions from one part of the body more than from another.

If the body were a solid mass, the molecules of which could not shift their places as in a liquid, and if the heat were diffused through the solid by conduction from within outwards, it is obvious that if it were irregularly clad it would cool unequally. If the head or leg were uncovered, it would, in a cold air, cool through and through to a much lower temperature than a well-covered part. This, were it not a link in the argument, would be too obvious to require being stated.

But the actual structure and property of the living body are well known to be such, that a part of it may be entirely denuded, even in an intensely cold atmosphere, if the rest is well covered up, and that for an indefinite period of time; and yet, within the very surface, the exposed member will throughout have the same, or within two degrees of the same temperature, as those thickly clad.

The office of thus equalising the temperature throughout the body, we have the strongest evidence, is performed entirely by the circulation of the blood; and its instrumentality, we have strong reason to believe, is twofold. The warm blood travelling to the part brings heat to it in a manner similar to a hot-water circulation. Of its agency in this way we are quite certain. It is, indeed, a necessary consequence of its being warm, and of its coming to the spot. But we have strong reason for thinking, indeed we may almost be certain, that the blood also causes the production of heat on the spot, by bringing with it elements which, when the fluid enters the capillary system, or minute vessels, are made to hold commerce with each other, the result of which is the production, with other compounds, of carbonic acid and water. These, in forming, generate and give out heat, though they themselves, or at all events the greater part of the carbonic acid, continues in the blood till it arrives at the lungs. Some may vary the exposition of this last process according to their views. But on this fact all agree; namely, that the temperature of the part is maintained by the circulation of blood to it.

Hence, we find, that in proportion as more *heat* has to be developed in a part, more *blood* must flow to it. Whenever, from any irritating cause, a part grows *hot,* our sight assures us that it is at the same time more full. There is then, invariably, a corresponding increase of blood in it. Now, the very same thing must, and does result, when a greater abstraction of heat takes place from any one part than from the rest of the body generally, if it is continued for any length of time. It is obvious, that to prevent that part from falling below the others in temperature, more heat must be supplied to it ; and *we know, that to effect this there must be a corresponding increase of circulation to the part.* But is the *difference* between the rates at which heat is allowed to pass off from different parts of the human body in all cases too small to be worthy of notice ? The very contrary. It is oftentimes enormous enough, to cause any reflecting mind to wonder at the ability of the circulation to counterbalance it. Thus, when the whole body is clad with a heavy load of clothing of several layers, and the face is exposed to a piercing wind, the conduction of heat from that part must, we may affirm, be ten times what it is from any other equal surface, perhaps much more ; yet, as before stated, immediately within the surface it is as warm as the others ; and if a person so circumstanced shall suddenly withdraw from the chilling influence by coming into a warm room, the burning glow in the part which had been exposed informs us of the large quantity of heat that was being supplied to it before, but which, under the rapid abstraction, was not perceived. The flush also informs us of the corresponding flow of blood to it. But it may be said there is some peculiarity in the circulation in the face enabling that part to endure such exposure: not so; it is merely a work of habit. The Asiatic, especially the Hindoo, by custom habituates his legs to endure exposure to cold; and in the winter air, which in the western provinces of India is very piercing, he may be seen walking with his legs, and often his feet bare, over a frosty ground, but with his head and face wrapped round and round with a bulky cloth or blanket, so completely as to leave only his eyes visible. We often see a line of native boatmen loaded with clothing about the head, but with their legs and feet naked, and every few minutes wading through fords, allowing the water afterwards to evaporate away from their legs in the keen wind, with an indifference to the cold as to those parts, as surprising to us Europeans as is the ludicrous sensitiveness of their heads and faces.

Thus we perceive that while we in England expose our faces to damp and cold, almost to any degree, without uneasiness, but are very sensitive of their application to our feet, the Asiatic cannot endure this exposure of his head and face, but is indifferent towards cold and damp when acting upon his legs and feet.

It is not necessary, though it were easy, to adduce other facts to prove that the choice of the part which may be exposed is altogether arbitrary, and is determined by custom. It is, indeed, not uncommonly argued, because one part can learn to endure exposure, so also might another. This is true in one sense, and false in another. Any one part of the body may be brought to endure a greater exposure than any other; that is, any one may be made the chief outlet of heat in preference to any other: but it cannot be exposed *as well* as the other. As the one learns to endure exposure, the other must be covered up; unless indeed, as before stated, the person acquires, at the same time, the power of supplying heat equal to the demands of the double outlet. Hence we perceive the error, often fatal, of those who think, because a child can endure to have his face uncovered, that nothing but habit is required to accustom him to go with denuded limbs; and thus we have to lament the ignorant infatuation of civilized (?) life, which not only countenances, but requires, females to cover up their bodies at one time of the day, and at another, and that the coldest, to increase three or four fold the denuded surface; as when a female is in, what by a misnomer is called, full dress. We are wont to be surprised at the hardiness of the Scotch and Irish peasantry, of whom many go barefoot and bareheaded; but the cutaneous accomplishments of ladies are as much superior as are their mental: they could not, indeed, endure exposure to the same amount of cold; but they learn to bear what is far more trying—a sudden and diurnal transition. It is probable that it is the *periodicity* of the exposure which makes it at all possible to endure the transition, even though the exposure takes place at the wrong hour for *warmth*, however preferable to some with reference to *light*. There is reason to think that in this case, as in many others, by a providential arrangement, the effects of our follies are in a considerable degree controlled; and that elements are periodically each day stored up in the blood as materials, or as it were fuel, for producing heat at the time it is wanted. It is difficult otherwise to imagine how, in the same atmosphere, so different degrees of exposure can for many hours be endured. This hypothesis appears confirmed by the fact, that if a lady suddenly changes the period, and is covered up at night, she may feel even oppressed by heat, and in the morning she could not endure the undress, or rather "full dress," without feeling, and most probably taking, cold. What is this, but that at night her capillary system, especially of the exposed part, by periodical habit developed more heat, which, when covered in, was felt to be superfluous, and in the morning the production of heat was by habit at its minimum; hence the sense of cold in the same parts from the unwonted exposure at that time? But when we discover these protective arrangements of a watchful Providence, let us not in consequence give countenance to so reckless, and in too many cases suicidal, a trespass upon them. It may be worthy of reflection whether, in some cases, a tendency to severe headaches, and in others to eruptions on the face, may not be favoured, if not induced, by this fluctuating exposure. For, notwithstanding the provision just noticed for varying the supply of heat, there can be no doubt that the head and face, during the day, have to throw off much more heat than at night, when other parts are also exposed. There may be some constitutions in which the production of heat becomes permanently instead of periodically increased. In these the developing and dissipating of it will be divided over the whole exposed surface *at night*, but *in the day* will be concentrated over the head and face. In these cases it were no wonder that so great and repeated increase of local action should produce, or at least aid other causes in producing, headache and cutaneous affections.

CHAP. III.

ON DETERMINATIONS OF BLOOD TO THE HEAD.

We are now led to the most important part of our subject — the effect of long-continued exposure of parts in producing local determinations of blood; and we shall be compelled to do violence to a prejudice which has grown up with us from our youth, which exists in every mind, and which has been handed down by our forefathers in the maxim, to keep the head cool and the feet warm.

This can scarcely be uttered, before we must feel ourselves interrupted by the interrogation, " Can so prevalent and ancient an opinion be incorrect; and does not all experience tend to confirm it? " While we concede all that this question demands, within a limit as to time, we must affirm, *without that limit*, that the opinion, however universal in England, is wholly incorrect, and all sound evidence therefore against it.

The error has lain in our having, in this case as in many others, failed to discern between the primary and the secondary effect of a cause; in our having always fixed our sight upon the great but temporary influence of its first action, and having not proceeded on to trace out its gradual, but permanent, action. That this should be the case we shall not wonder, when we reflect upon the widely prevalent mistakes respecting the action of strong spirituous cordials. We find that their ultimately injurious, is by multitudes overlooked in their primary stimulating, effect.

In the case before us, it is undoubtedly true that the application of heat to a part, or, what is the same in nature, the confinement of the heat passing out from it, produces a determination of blood towards it; and, on the other hand, that the application of cold, or, in other words, the sudden abstraction of heat, causes a recession of blood from it, and each of these is amongst the most powerful measures we possess for producing the one effect, or the other. But though a determination of blood *to* a part in the one case, and *from* it in the other, may thus be repeatedly produced, and even kept up for some time, *it is, after all, a temporary effect only; and if the employment of the means be persevered in, not only does the primary effect subside, but an opposite one is established.*

This important proposition will be found universally true. Not only may it be placed beyond a question by argument founded on established principles, but it may be proved by the evidence of numerous facts.

In the first place, when all the body is well clad excepting one part, and that is kept exposed, we know that more heat must be drawn away by the air from that part. This unequal exit of heat would happen just as much to a heated metal statue partially uncovered as to the living body, and is an effect, as already stated, of an outward cause over which the body has no control. If the air without is much colder than the body, the difference, in the quantity of heat going out of such a part, and of one of equal surface which is covered, is very great. If the uncovered part is exposed to a winter wind, often 80° below the temperature of the body, and the rest be fully clad, the exit of heat at that part becomes prodigiously greater than of any other.

Secondly, This very unequal abstraction of heat would rapidly cause a fall of temperature in the whole member or mass under the exposed surface, which even in a summer air would cause, in a very short time, a death of the part, unless more heat were distributed to or developed in it.

Thirdly, The only, and the invariable means by which more heat is brought to and produced in a part is by an increase of the circulation of blood to it. This we know from theory. We know it also from fact. Thus, the increase of local heat in flushing, blushing, and in inflammation, is invariably accompanied by an increase of blood in the part.

Fourthly, When a habitually greater exit of heat calls for a habitually greater supply, we may affirm from theory that this supply can only be effected by an increased determination of blood to the part. Since exposure of the part to cold necessarily involves with it a greater exit of heat, such a local exit of heat does, or does not, lead to a greater determination of blood to it. When it does not, the part, in obedience to a physical law, begins to cool down, and the health instantly suffers; as when a person who is not vigorous leaves off an article of dress; or if a lady should add to the imprudence of her evening dress, that of going in it into a wintry air with her arms uncovered: in her case, in most instances, there would be no time for an increased determination of blood to her limbs; they would be chilled, and disease in some vital organ be excited. Whenever, then, a local abstraction of heat, continued for any time, does not establish an increased determination of blood to it, the health or life must suffer. If they do not suffer, then to a certainty is an increased determination of blood established in the part. Thus the milk-maid, with her arms exposed to a frosty wind, requires a prodigious quantity of local heat to be developed, to maintain their temperature equal to that of her body generally. The arms of a statue by her side, heated red-hot, would in the course of an hour be colder to their centre than hers an eighth of an inch from the surface. How does nature supply this great local demand for heat? By increasing the flow of blood to the exposed surface until it becomes ruddy, and even purple, and the skin turgid with blood. While the air is rapidly carrying off the extra supply of heat, she is not conscious, nor should we be on touching the arm, that more heat is passing off from it than from any other equal surface of her body which is thickly clad; but let her enter a room at 50°, and uncover some portion of the body thus clad,—it will soon be chilly to her feelings, and cool to our touch; while her arms, in the same air, will now glow with heat, and feel hot to our touch.

This fact establishes three points very plainly: first, that a prodigiously larger quantity of heat was being developed in the arms than elsewhere; secondly, that this larger supply of heat to the part exposed was provided by a proportionally larger supply of blood, as shown by the fulness and ruddiness of the limbs; and, thirdly, that the continued exposure to cold, by creating a local need of more heat, established the local determination of blood.

These points, then, thus established by a simple and familiar case, exactly accord with what we have seen might be predicated upon physiological principle. Again, in an assembly of men whose occupation is in-door, however active, if one should enter whose calling places his face all day in a current of wind, as a coachman, guard, or railway conductor, we know that any one could pick him out directly, as the person who was always in the open air. *Experience* would assure any one which to select; but the physiologist should add to it a knowledge of causes. From the faces of the in-door men comparatively little more demand of heat is made than from other parts, not enough to call for a thronging of their faces with blood; but in his case, the rapid conduction away of heat requires every vessel of his face to be fully employed in bringing blood. Not only could we thus select him from the rest as the one who had extensive dealings with cold winds, but, if we had to decide between his face and any other part of his body, his florid countenance would tell us that *it* was the great outport, crowded with a busy throng of particles in full commerce at this the free port of the skin. By comparing his full and ruddy face with his white legs, we might, were we Asiatics, unacquainted with the English habit of covering the latter and not the former, affirm, if rightly instructed, that the man before us could not be a follower of the Hindoo practice already described; for then would he have had a pale face and ruddy legs. If fashion in her unaccountable caprice should, at any time, be pleased to rear a generation so clad, she could, without a doubt, transfer the emblem of health, the ruddy complexion, from the upper to the lower end of the body; from the face to the feet. Such a transfer would be seen in the case of the Hindoo, in cold weather, were his skin as thin and his complexion as fair as ours. The feet of Scottish girls, already pink, would become scarlet if their faces and arms were covered up, and their feet remained the only free ports of heat. At present, the busy transactions between particles and the exportation of heat, are divided between the limbs and the face; and surprising it is that the digestive operations of the system are extensive enough to keep so many open ports at all adequately supplied.

Again, if we part the hair of a bushy-headed person, and take a view of his scalp, we shall find it, even in the full-blooded vigour of youth, quite white. If we should then shave his head, and by degrees accustom him to go bald, or if we wait until time does this, we shall find the skin of his head become pink, and sometimes quite red in cold weather; an effect true to its cause,—the habitual exposure of the part to cold.

Such, then, is the secondary and enduring effect of exposure of a part to cold; namely, an increase of vascular action in it.

Fifthly, If we inquire into the secondary and enduring effect of the *confinement* of animal heat in a part, as by covering up, we shall find it the reverse of the former. The more a part is covered up the less, *by degrees*, becomes the vascular action in it. The chilliness of the head of the Asiatic in cold weather proves the little action in the skin there; and this is brought about by his habituating himself to wear a bulky turban at all times, even in the hottest weather. A more striking proof could not be adduced. Again, if we change the employment of a man whose Mercurial occupation has hitherto perpetually wafted him through the air, and given him the ruddy countenance, and if we keep him within doors, he will lose much of his facial vascularity. Were we to cover up his face we should in time make it as pale as his arms; hence the pallid face of the nun, who, in superstition or in spleen, long since *took the veil.* It is this diminished action from habitual warmth which causes the brother of the milk-maid, though as vigorous as herself, to have white arms under his thick coat, while hers are red and turgid; and which will be further proved, if we bring both of them from the winter farm-yard into the shelter of a room, and place their arms, uncovered, side by side. He will soon complain of their chilliness, while she will speak of their glow; his arms will have become cool to our touch, hers hot. Not to multiply evidence needlessly, the covering up or confinement of the heat of a part, in the end, as universally diminishes action in it as does its converse, exposure or increased abstraction of heat from it (if it can be endured), produce an increased determination of blood to it.

Such are the *secondary* and *enduring* effects of habitual cold, and warmth, acting locally. Let us now consider the *primary* effect of these agents when likewise acting locally. As already remarked, we shall find them the very opposite to their secondary effects.

First, as to the application of warmth, or even the lessening of the accustomed exposure to cold. The ruddy arm or ruddy face on entering a warm room will, for a time, be flushed doubly red. It is when we subject the head and face of an apoplectic man to the heat of a crowded room that he falls in a fit.

Of this *primary* effect of warmth more need not be said. We have been always sufficiently impressed with it: too much so indeed; for it has universally obscured from our view the *secondary* and *lasting* effect of it.

A few words will suffice upon the primary effect of cold. If we expose a part of habitually small vascularity from covering up to the action of cold, its *primary* effect will be to lessen still more the action in it. The chill, from abstracting heat where there is not a supply ready, we know so acts upon the nervous system as to deaden its sensibility, and to produce what is called (whether rightly or not) a constriction of the vessels of the benumbed part; as at all events to lessen their fulness, and to send the blood elsewhere. With this effect we are abundantly familiar. Where the action in a part has been above par it is a remedy of great value; as in an apoplectic attack, where the rapid abstraction of heat is, next to depletion, (and in some particular cases superior to it,) our most powerful measure for lessening action in the head. Its power is equally set forth when acting perniciously; as if the apoplectic man, already endangered by the heat of an assembly acting on his head and face, should have entered it with wet feet, and be standing on cold stones. In this predicament he would be subjected to two causes opposite in nature, but acting in the same direction,—to the primary action of heat on his head, and the primary action of cold on his feet; the former inviting blood from other parts to his head, and the latter supplying it; and their united action would too probably seal his fate. We need not go further. This *primary* effect of cold, as of heat, has been also abundantly kept in view. Hence the prevailing maxim of keeping the head cool, and the feet warm; a maxim excellent when qualified by a conditional element—time. But by failing to discern that the object we inculcate in our maxim is the primary but not the enduring effect of our measure, harm, as well as good has resulted from this unqualified and universal recommendation of it.

That this is true might be conjectured from what we have already seen. It will be established by now studying the *combined* action of the primary and secondary influence of warmth and of cold.

The Hindoo flies in the face of our English maxim; and in a climate which we consider, and which is in its tendency eminently productive of cerebral affections, and in which, on account of them, the treatment of disease in the European, especially in European children, requires the greatest vigilance,—in such a climate, then, *he* takes the opposite course to *us*, and habitually keeps his feet cool and his head warm; loaded, indeed, with a turban, which often, and that in the hottest weather, contains more cloth than all his other garments put together. In a climate tending so much to head affections, were the views which have established our English maxim as unconditionally true as we have hitherto been taught to think them, such an inversion of the head and feet, as to our view of clothing them, would be as fatal to the Hindoo by driving the blood to his brain as would hanging with his head downwards,—which, by the way, he can do, and often does as a penance, for a surprising length of time; so thoroughly has *his* system of clothing established an *enduring* determination of blood to his heels, and away from his head.

My own observation of the different races of India, and it has not been trifling, has led me to consider them in a remarkable degree exempt from affections of the head. I have known elderly men, upon a sudden change of fortune from want to affluence, grow rapidly fat, and very corpulent; and have watched them, loaded as to the head with a turban which has increased in bulk with their wealth, cooling themselves by standing for a long time knee-deep in a river!

If we compare together an European and native labouring under the same fever, in the former we shall find an unceasing effort, as it were, of the blood to flow to his head; in the latter very little of such tendency, even when he is a man plethoric and corpulent, and of sedentary habits. It is not denied that there is a certain amount of head disease amongst the natives; but it is surprisingly small. Most willing to assign to their abstinence from alkohol its influence in this exemption, the singular chilliness of their heads, as well as all the facts and arguments above set forth, shows the amount of action in the head to be not only small, but less than in other parts; and that the chief cause producing this is their habituation from infancy to a great deal of wrapping of the head, and to a denuded state of the legs and feet.

If we compare with this the case of the European child, in which by the way alkohol does not enter as an element which could affect the question, we shall find in it a remarkable confirmation of the proposition above maintained. The great tendency to attacks in the head in European children in India has been already mentioned. The tendency of the English child in England is much greater than of the native in India, but the English in India far surpasses the former in liability to head disease. A main cause of this, and I regret to say one that I fear cannot be removed, will now appear. A child needs always, even in the hottest weather, to have its feet and legs, as well as its body, well covered, to guard it from moschetos. Often the invasions of these insects render it necessary for children to wear boots. This throws the burden of discharging a great quantity of heat, upon the head in such a climate; which discharge the action of the *punkah* is powerfully instrumental in promoting. This fan wafts air backwards and forwards over the head during the whole day; and it is well when careful parents continue it through the night. Even were the feet not covered, the greater proximity of the head to this large fan suspended from the ceiling would cause the current to set chiefly on the head. In this manner there is kept up a constant drain of heat from the head, which establishes, according to an unerring rule, a proportionally great transfer to it of heat, and to produce this, a flow of blood. The pink skin of their hapless heads may often tell us of their critical state, and ought, perhaps, to produce a congenial blush in the nation whose political system gives birth to, and buries, thousands of its offspring in so ungenial a climate. Upon a child being kept away from the punkah for some time, I have often noticed the effect produced upon the head. The local accumulation of heat which takes place, not unfrequently produces its *primary* effect—a *flush*, or further rush of blood to the part. It is true that nature, which bears with much, will in many cases endure this state of things to be frequently repeated ; but it is likewise true that it is more or less injurious to all, and fatal to many ; and it is a chief cause rendering necessary the early departure of European children from that climate.

We have here an instance of the secondary effect of abstraction, followed by the primary effect of retention of heat, both acting in the same direction. The habitual abstraction of heat from the head made *it* the place of chief action, the busy out-port of the skin, and produced as its consequence the secondary or enduring effect of a determination of blood to the organ. Upon the foreign demand for this heat (that of the punkah) being suddenly put an end to, the local excess, or glut of it, has an immediate tendency to produce that primary effect of local heat,—a temporary stimulation of the nerves and vessels, and consequent fulness of blood in the part. This occurring upon a part already fuller than natural will readily account for the injurious consequences.

I have noticed the same fact repeatedly in adults ; though in them, excepting when already ill with fever, it is less marked. In cases of bald-headed, elderly people, I have had reason to think their indulgence in the cooling influence of the punkah conduced towards that determination of blood to the head to which elderly people in India are very prone. In that country I cannot but consider it very unsafe for dignitaries of the church, and of the law, at their houses to make their bald heads the great outports of the accumulating heat, thereby establishing a current of blood towards them, and on going into public suddenly to close the gate by a wig, and thus add the danger of the primary effect of excessive local heat to that of the secondary effect of an excessive local abstraction of it. To such persons the climate of India has been remarkably fatal. In their case the evil is aggravated by their dignity not permitting the use of the cool cotton dress, in public, which would allow more of the animal heat to be liberated from other parts of the skin, and diminish the burden thrown on the skin of the head.

The same fact, of a determination of blood to a part being kept up by a local abstraction of heat, I have observed in certain cases, where a prolonged employment of cold applications had been rendered necessary to subdue obstinate action in the head. For a time these measures are very useful, so long as the primary effect of the cold lasts,—that of checking action in the part; but by degrees, in some cases, when they cease to be efficacious they may be found to have established an increased action in the part; for if they are desisted from, they are followed by a burning heat over the head greater than ever. In such cases I have found it necessary to continue the use of them unremittingly, until the patient could not bear the cold of the wetted cloth under a fan, which is a favourable symptom, indicating a diminished supply of heat, and therefore of blood in the part; and it has then been necessary to reduce the cooling very gradually. In one case,—that of an officer attacked with an epidemic fever, who had suffered a gun-shot wound in the head some years previously,—this effect of cold applications was remarkable.

The extent to which an increase of action in a part may be established by habit, enabling it to endure for a length of time an abstraction of heat to a prodigious amount, is illustrated in the extraordinary practice of the inhabitants of certain elevated valleys amongst the Himalaya mountains.

Mothers habituate their children from their infancy to endure the action of a current of water flowing for many hours over the head. They conduct, from mountain streams, small shoots of water through hollow canes, and placing the children upon grassy beds, with their heads below the ends of the canes, they allow water to be projected from them upon the head, and to run over the whole of it excepting the face. A child falls quickly to sleep under its influence, and is left to sleep for several hours. I have been surprised at the comparative coldness of the stream.

We cannot doubt of the effect of this long-continued local abstraction of heat in causing an increased development of it in the part, and of consequence an increase of blood in it; and we are left to suggest a reason (beyond the mere purpose of putting children to sleep) for this practice, and for its not proving fatal by such habitually increased action. It is a curious fact, that the practice is not met with any where in the plain country of India, being, so far as I am aware, entirely confined to the Himalayas. We have to look then to some point connected with elevation for a cause; and do we not find one in the following? Man is an animal all of one genus, and that genus formed to inhabit the plain surface of the earth under an atmospheric pressure equal to that of thirty inches of mercury. It has been well shown by the experiments of Keill and others, and well argued by Abercrombie, that the contents of the head are not directly under the influence of atmospheric pressure, there being no part of the skull nor any membranes covering its apertures in any direction outwards which are compressible, or at least so far compressible as to yield to any difference of pressure equal to that arising from the tendency of the fluids to gravitate out of the head. Hence we must view the retention of the blood in the vessels of the brain, against their gravity, to be not so much due to the tension of the skin and vessels of the rest of the body, supporting the column of blood like a distended bag, as to the atmospheric pressure over the soft bodily surface, keeping the blood in the head in the same manner as the mercurial column is supported in the barometer, and would press against the summit of the tube were the weight of the mercury not so great, or its column shorter.

The interesting and important consequence of this provision is an unvarying supply of blood to the brain. The soft and fluid contents of the skull being nearly incompressible, and the skull itself neither extensible nor compressible, the consequence is, that being kept full by the atmospheric pressure on the body acting upwards, it cannot be more nor less than full. Hence it has been found, in an animal drained of its blood as far as possible, that the vessels of the brain remain full.

But while we fully discern the truth of the fact that the quantity of blood in the brain is invariable, so that if extravasation of blood take place any where within the head, by over-distension of the arteries, the fulness of the cerebral veins must be reduced to make way for it,—with this fact clearly before our view, we must yet perceive that the brain is not thereby relieved, at all, from the influence of atmospheric pressure. It is nearly as much affected by variations of pressure, as if it were contained, like the rest of the body, within soft parietes; and for the following reason.

The influence of the blood as a stimulus upon any organ it is visiting, depends, not upon its *quantity, as such*, in the vessels of the organ, but upon the quantity causing, by its distending the vessels, a firm pressure of the fluid against their inner surfaces. It is therefore firmly pressed against all absorbing orifices, and nervous points distributed over their surfaces. In this way, doubtless, the increase of the quantity of the blood in a part acts as a stimulus to greater action. Again, when the organ is contained within a partially resisting membrane, as that covering most of the abdominal viscera, an increase in the bulk of the blood entering it must cause pressure against the parenchyma or substance of the organ in every part, so that, if it were compressible it would be condensed. In this way may an organ be oppressed with blood, —be congested.

Now these effects, of stimulation and oppression, are not felt in such organs clad with an extensible membrane, until the quantity of the fluids present is increased, for each vessel can swell under the distension to some extent before its parietes feel the pressure of the fluid against them to be oppressive; but these effects, in the case of the brain, may and do result, without any variation in the absolute quantity of blood in the head. All the effects, of both increased and diminished pressure, may be produced on the brain, without the quantity of the blood present varying in any degree. The case is very similar to that of the hydrostatic press, the pressure against the inner surface of which may be varied in any degree by loading the handle of the pump; yet not the smallest variation in the quantity of the fluid in the press need take place, provided the piston be prevented from rising, — *i. e.* the parietes of the press from expanding.

It might then be affirmed with confidence, even by those who have not experienced the effects of lofty heights, that for every inch the mercury falls in the barometer, there must be a proportional loss of natural pressure of the blood against the coats of the cerebral vessels, and of one part of the brain against another. This is a fact in physics against which there can be no contending. Now this deficiency amounts, at the heights commonly inhabited in the Himalayas, to many times the difference of pressure between an erect and horizontal position of the body. Yet how great is the effect of this! On the one hand we know, where much blood has been lost, that a mere raising of the body into an erect position will often determine the fate of the person, causing immediate death; and on the other hand, where the pressure against the brain is too great, laying an apoplectic person horizontal will often cause the attack to prove fatal.

There can be no question, then, but that the lack of pressure on the brain at mountain heights must be made up by increase of vascular action. I care not here whether the physiologist shall say it is the heart alone that can do this, or, with myself, shall believe that the vessels of the parts will also be actively concerned in keeping up the local tension.

Now can we do otherwise than perceive, with respect to the naiad life to which the little Himalayans are habituated,—since it tends, in obedience to an invariable law, *to produce local action in the head* to meet the local demand for heat,—that this increase of local action tends to qualify them to reside, and to labour also, at great heights, by affording to the brain a proper amount of pressure to make up for the loss of barometric pressure; and that, by establishing this requisite action from infancy, the heart and vessels of the head are not called upon suddenly to excite themselves, the tendency of which, it might be shown, would be to produce extravasations of blood and certain other curious effects? This view is confirmed by the fact that many strangers visiting those great heights do actually feel head symptoms *of a certain kind* aggravated, instead of being relieved, as might at first sight be expected, by the diminution of pressure. This last fact opens to our view a highly interesting and important field of observation; but one much too extensive to be entered upon here.

It does then really appear that experience, or some instinctive guidance, must have taught mothers to venture upon a treatment of their children very bold, but very correct and very philosophical, did they but know it; a treatment which in its nature must tend gradually to establish from infancy that increase of action in the head which is indispensable to balance the deficiency of pressure, and which cannot be so safely or completely acquired afterwards. Surely this fact, of a want and the provision for its supply, falling thus in juxtaposition as to local circumstances, is very remarkable; and may take its place in our impregnable array of evidence, establishing the true nature of the enduring effects, on the one hand of a local abstraction, and on the other of a local retention, of heat.

We have by this time seen, in too many and various lights to admit of a doubt remaining, the truth of our proposition, that the secondary or permanent effects of a local abstraction, and of a local retention of heat, whether by cold and by warm applications, or by exposing and by covering up a part, are the opposite to their primary effects; and that these secondary effects are of the highest importance, although they have been commonly hidden from our view by the primary.

Let us now endeavour to draw from all the preceding evidence some important practical inferences. It is not uncommon to see a bald-headed elderly person so rigidly observing the common rule of keeping his head cool and his feet warm, that even in the coldest weather he will ride in a carriage, or walk about the cold passages of a house, with no covering over his bald pate, but with thick woollen hose and gaiters over his legs, and perhaps cork-soled shoes upon his feet; yet with all these precautions still finding himself singularly prone to undue action in the head. If we examine the head of such a person in cold weather, we may see a pink flush pervading the whole of its surface. He himself must notice it; and it doubtless alarms him, and renders him all the more careful to keep his head cool, and perhaps to add some additional covering to his legs and feet.

When such a bald-headed person enters a heated assembly, we may invariably perceive him to be the individual most oppressed. He is the one who is in most danger of falling down apoplectic.

Can there be any doubt, as he grew bald, and the head became the chief outlet of animal heat, that the certain consequence of this, — increase of vascular action, was quite enough, independently of any other causes, to alarm him by slight symptoms of fulness in the head? These, then, would induce him to wear his hat as little as possible, — to learn perhaps to sleep without a nightcap, and to load his feet and legs with additional covering. Now each one of these acts, in its *primary* effect, would appear to do good. It would relieve the symptoms a little, by diverting the action from the head for a time; but by degrees their secondary effects would be established, in an increase of the action in the head. Thus the covering up of the lower extremities would, we know, at first cause a determination to them; but very soon the action would fall below par, as we might be satisfied by their chilliness if uncovered, and by the striking contrast of their pallid hue to the ruddy colour of his face.

In fact we are certain that it must be so. Less heat being let out by the lower extremities, as they are thickly covered up, more is available to supply the extra demand arising as the head becomes denuded. The certain consequence of less exit of heat from the legs is a reduced development of heat there, and therefore also lessened vascular action; hence the paleness or tendency to action below par in the feet, and to a transfer of the excess elsewhere; hence a twofold cause increasing the action in the head,—the increased local development of heat in the head effected by an increase of vascular action in it, and the transfer of confined heat and action from the limbs below. The cooling of the head produced, for a short time, its commonly observed primary effect of lessening the action there; but when persevered in, the loss of heat could only be endured by establishing *the increase* of action there. Otherwise the head would have cooled to the centre of the brain, in obedience to the law of conduction of heat, which it has no power of resisting, and which it can only obey, and at the same time preserve its heat, by inviting the aid of more blood.

Thus many a person, under the impression that he is diverting the blood from his head, is following a course which, however ancient and however high may be the authority supporting it, we may affirm with confidence has such an effect *only for a short time;* and THAT ITS ULTIMATE, AND ENDURING EFFECT, IS AN INCREASE OF ACTION IN THE HEAD. As soon, then, as he enters a heated assembly, there is suddenly superadded to this established increase of action from the local abstraction of heat a further increase at the moment, as the primary effect of the retention of heat, the air of the room not carrying it off quickly enough. We need not, then, wonder at the too frequent consequence of their combined action,—a fit of apoplexy.

With such a view of the case, we must desire a change of measures. To adopt an opposite course were far more easy, safe, and effectual, in the first instance; but we must not despair, even when the injurious practice has continued long. Since undue local action is inseparable from the great local abstraction of heat long continued, the desideratum obviously is, to put an end to such abstraction of heat from the head, and even to reduce it below par, by diverting it, and the active business attending it, away from the head, and inviting it to some other outports. It were well, in fact, if so great a change could be effected, as that our invalid should become as chilly about the head, by diminished action there, as the native of India, who has, by some surprising instinct or sagacity, been brought to adopt a course we cannot doubt, both upon sound principle and from its effects, the best for guarding him against head affections, which would otherwise prevail much more in such a climate. It were well if our English invalid could be brought to wear all the woollen in his hose and gaiters in a triple cap covering his head and face, and like a Hindoo or a Highlander to go with his legs bare.

But if we were to attempt this at once we should, too probably, destroy him by the primary effects, before we could establish the secondary. We must proceed with the greatest caution. We may have often, during periods of cerebral action, to retrace our steps, and to cool the head and warm the feet; but if we perseveringly return to our treatment, inspired with the confidence that it is built upon a sure foundation of principles, and strongly cemented by facts, we shall assuredly find it do us credit in the end. In many a case we may, though perhaps with several retrogressions, at last establish, not quite a Hindoo winter costume, but yet a great change in our patient's dress. We may accustom him to a wig of double warmth, covering the sides of his face as well as his head. We may gradually rid him of his gaiters, and if he is an active generator of heat, also of heavy stockings, substituting some light fabric for his fleecy hosiery; or we may transfer the heat-developing action which has been diverted from the head, to the surface of the body generally, by accustoming him to lay aside his great coat.

Our treatment, however, will no doubt assume its most valuable form as a preventive, by warning a man of full habit against the baldness of advancing years; directing him to accustom himself to the use of a wig, and to plenty of wrapping about the face; and while we of course caution him against *sudden* chills in his feet, we must exhort him not *to habituate* them and his legs to the warm treatment hitherto adopted.

We must, in short, keep the conclusions to which we have arrived by an ample investigation of the question continually in our minds, and their exemplification in the Eastern costume constantly in our sight. We shall then find our labour rewarded in the satisfactory results of many a case.

The advice here given may not always be followed with safety by the invalid himself; but, under the watchful guidance of a qualified adviser, the change of habit may be entered upon with perfect security. The sagacious practitioner will observe the moment, when a retrogressive step is necessary, and when the course of habituation of the right kind may be resumed.

A few words are called for respecting the treatment of children. From all that has been said it will appear that the practice which has of late years prevailed so much,—that of habituating the head from infancy to cold, by keeping children without caps,—has a tendency of the opposite kind to that purposed by it. In the bulk of children little injury may result from it; but it cannot in any have a good effect, excepting in the few in whom there is not action enough in the head. With respect to those who have already a tendency to cerebral action, such treatment, valuable on account of its *primary* effect at the moment when the head is attacked, must, when persevered in, have its secondary and pernicious effect established, and must, to a greater or less extent, be heaping up the mischievous action it is intended to allay. Moreover, by use the value of its primary effect as a temporary agent is lost, or greatly diminished. On the other hand, by habituating the head to much covering, we shall dispose it to moderate its action. Our treatment, to however small an extent it may be effectual, will act in the right direction ; and when an attack comes on, by only removing all covering we shall command the primary action of cold in no small degree, and may increase it by cold applications far more, than if the child had been habituated to a constant and large abstraction of heat from the head.

In these cases, as in those of elderly persons who have habituated themselves to making the head the great outport of heat, much discretion would undoubtedly be necessary ; but we ought not therefore to be diverted from our ultimate object. Where there is a tendency to increased action in the head, we should by degrees accustom a child to have its head always well covered, and in proportion, lighten the clothing over other parts, especially over its arms and legs, avoiding, of course, more cooling, than the case demands to balance the covering of the head.

Sufficient has, I trust, been said to set forth the great importance of a change in our system of covering those invalids, both old and young, who are already, or are likely to become, prone to undue action in the head. The subject is of so great importance that it will be well if these pages shall invite controversy, when the truth of the position taken will stand out in more prominent relief.

Lastly, the question suggests itself, — is then our English costume productive only of evil, as tending to increase action in the head, and has it no advantages? To this we may reply, — that in childhood, where from predisposition there is a tendency to undue action in the head, and in advancing years, especially as persons grow bald, we may consider our system of costume as one of those causes, adverse to longevity, which tend to counterbalance our many advantages, and raise our average of mortality more nearly to that of the world in general. But in the case of the bulk of the population, we may believe a good deal of action in the head to be of no small value.

When the operations of the mind call for much exertion of the brain, we see, in the flushed countenance, how much blood is wanted in the head. We know that stimulants applied to the nostrils, and taken into the stomach, have the effect of calling up more action in the brain, and at the same time a flow of blood towards the head to support the function of the brain. In short, we are well assured that the blood is the natural stimulus to the brain. As on the one hand faintness and a loss of sense attend action diminished below par, so do we find a full amount of action there, conducive to mental and bodily energy, provided there is not, with the quicker motion of the fluid in the vessels, an undue amount of pressure. Hence, a cause, which is not of transient operation like stimulants introduced into the stomach, but which keeps up unremittingly a large transmission of blood through the organ, must powerfully conduce towards energy of mind and body. If, then, we were informed of two nations, in one of which the practice was to go bare-headed, and even bald-headed, and to clothe all the rest of the body well; and in the other to load the head with a profusion of cloth at all times, commencing in infancy with a wadded cap covering the ears, and to leave the legs bare even in cold weather, — we ought from these premises to form the following conclusion respecting each. Of the former we might affirm with confidence, that such a costume would have a greater or less tendency to induce too much action in the head, in the case of many; at the same time we might, though not confidently, yet with sound reason, conjecture that this costume would favour a general energy of mind and body in that nation. With respect to the other, we might feel assured that its costume would, as far as dress can have effect, prove in the greatest degree preventive of head disease; but we might shrewdly suspect that it would lend its aid towards inducing want of energy of body, and mind, in this nation. In comparing, then, the natives of England and of Hindostan, we have found head affections to be as little prevalent amongst the latter, as they are rife amongst the former; and with respect to the other points in the two categories, there is at least a curious agreement between the anticipations to be derived from physiological reasoning and the national characteristics of the two people. This, then, we may consider as one of the ways in which their climate may conduce to the apathy of such Asiatic nations.

The great importance of such a subject as local determinations of blood, especially when affecting the head, would have authorized a more extended disquisition, upon the doctrines hitherto received and those now set forth. It has been my object, however, rather to invite attention to the latter by brevity; little doubting when it is excited, on the part of the profession and of the public, that so serious a question in prophylactic medicine will be afforded the discussion and development due to its importance.

APPENDIX.

APPENDIX,

CONTAINING

AN EXAMINATION OF SUCH OF THE VIEWS

IN THE

"ORGANIC CHEMISTRY OF PHYSIOLOGY AND PATHOLOGY,"

BY PROFESSOR LIEBIG, OF GIESSEN,

AS RELATE TO QUESTIONS DISCUSSED IN THE PRESENT WORK.

I. NOTE TO PAGES 9. 11. 64. 89.

To an exclusive attention to the air of respiration is to be attributed a line of argument, in Professor Liebig's "Organic Chemistry of Physiology and Pathology," which, upon examination, we shall not be able to admit, and which will be found directly at variance with other views in the same work.

The subject is one of much importance to a sound physiology; and the great merit of many of his views renders it the more necessary that we should guard ourselves against the reception of such as are erroneous upon the credit of the others. In page 16. he remarks, "The capacity of the chest in an animal is a constant quantity. At every respiration a quantity of air enters, the volume of which may be considered as uniform; but its weight, and consequently that of the oxygen it contains, is not constant."—"Air is expanded by heat and contracted by cold; therefore equal volumes of hot and cold air contain unequal weights of oxygen." Again: "In summer and in winter, at the pole and at the equator, we respire an equal volume of air."—"It is obvious that in an equal number of respirations we can secure more oxygen at the level of the sea than on a mountain. The quantity both of oxygen inspired, and of carbonic acid expired, must therefore vary with the height of the barometer."—"Hence we expire more carbon in cold weather, and when the barometer is high, than we do in warm weather; and we must consume more or less carbon in our food in the same proportion." And again (page 25.): "In the case of a starving man, 32 oz. of oxygen enter the system daily, and are given out again in combination with a part of his body." In these and many other passages the author founds a series of propositions respecting points of great importance, upon the assumed fact that the capacity of the chest is a constant quantity. Thus the capacity being constant, he argues the *bulk* of the air respired is constant. The *bulk* of the air being constant, when the air is cold and dense, as in the winter, more oxygen must be respired than in summer; and more oxygen being respired, more carbon must be taken in to combine with it in the system: also, that on a mountain the light expanded air, containing little oxygen, will call for little carbon, and therefore little food comparatively to meet its demand.

We have, indeed, abundant evidence that in very cold climates, and to a certain extent in our winter, more food containing suitable elements for generating heat is called for than in summer. And there can be no objection to admit the reason assigned by the author, as having some influence; but upon examination it will be found very trifling.

In our climate the difference in the density of the air respired at different seasons does not, on an average, affect the quantity of oxygen taken in by more than a fifteenth; so that by inhaling only two cubic inches of air more, the difference would be made up. The case of mountain heights we have seen (page 65.) to be one in which, while the air is much *more rare* than in summer on the plane surface of the earth, much more heat is required, and therefore more oxygen, and that this is supplied by a respiration of *increased* volume; from which we perceive the author quoted to err in maintaining that our respiration is at all times of equal volume.

The facility with which the respiration is increased is such, that where it has to be increased by four, and even eight cubic inches, or be proportionally frequent, the difference is scarcely perceptible. It is not until we ascend to great heights, and add twelve or sixteen cubic inches to our breath, or respire one half quicker, that we begin to feel embarrassed; and by use the inhabitants grow accustomed even to this greatly increased respiration. Hence we may see how very little effect is produced by any differences of density in the air, on the plane surface, which can result from temperature; since a much greater difference of density, as on mountains, is readily compensated for by a fuller respiration. We must therefore perceive, and bear in mind, that nothing can be more inconstant than the capacity of the chest, in the sense employed by the author.

The case of a starving man, to which he alludes, might satisfy us of this; and would doubtless have been so viewed by him, had he reflected upon the readiness with which the bulk of our respiration may be varied. Thus it is in the last degree improbable, nay it is opposed to fact, that a man while starving breathes, as he says, the same quantity of oxygen as formerly, and expires the same quantity of carbonic acid. We cannot doubt that nature in her extremity husbands her resources to the uttermost. She reduces to a minimum the consumption of the fabric of the body. Hence we find during periods of inanition the respiration to be greatly enfeebled, excepting in cases of high fever. There are numerous instances of persons living for years upon a quantity of food utterly inadequate for their support, unless the carbonic acid they formed were reduced in proportion.

In this, and numerous other parts of the work, the author speaks of the oxygen inspired as an irresistible agent, the demand of which for carbon, as for prey, must be satisfied by food, or it will devour the fabric of the body. Surely it is far more reasonable to consider the action which goes on, to be entirely directed by the vital powers, which have a complete control over it, and that it is for the good therefore, and not the destruction of the animal, that the oxygen in the system tends,— that it is allowed, indeed, to enter the system. The consumption of the fabric in starvation does not, then, result from any ungovernable ravaging of oxygen, but is determined by the vital powers; it being the least of two evils that flesh should be lost, than that the animal temperature should fall to the point at which death must take place.

Lastly, were the capacity of the chest constant, *i.e.* the bulk of the air entering always the same, any difference in its density would not at all account for the very different quantities of oxygen which are required to correspond with great differences in the ingesta. The author strongly and rightly insists on this very different need of oxygen in many parts of the work, but then they are at variance with those views of the constant quantity of the respired air.

II. Note to Page 58.

This line of argument, establishing Lavoisier's theory of the production of water as well as of carbonic acid, and setting forth the near connexion of diet with the production of animal heat, also showing the absorption and discharge of fat by the lungs, with an attendant development of heat, was, as stated in the preface, written long since, though its publication has been deferred from time to time. It is very satisfactory to find it fully confirmed by two authors such as Doctor Collier and Professor Liebig, each of them doubtless, equally with myself, having been led independently to the same conclusions.

Indeed the view in question flows very naturally from the materials afforded us by the inquiries of Crawford, Lavoisier, Jurine, and others, together with those of the many experimenters, who have recorded careful observation upon the amount of the total daily discharges from the body by the natural passages. When the whole of such evidence is put together, the view opens itself to us at once; especially when, as in my own case, the mind is led into that train of thought by striking circumstances of climate, such as I have detailed. Doctor Collier has certainly established his priority of claim, by publication, to the particular view in question. From the date, since it must have previously occupied his mind, there can be little doubt that this immediate connexion of quantity and quality of the food with the development of heat must have offered itself to him first of all; and it is to be regretted that the view should not have been followed out still further, and published in a form more likely to be seen generally than in an appendix to a work unconnected with physiology. We must conclude that Liebig had not read the Doctor's remarks, otherwise he would of course have referred to them; and my own publication would have appeared without any notice of them, had they not recently been published in a suitable channel*, or had not Doctor Collier been so good as to draw my attention to them.

To Liebig must be conceded the merit of collecting the evidence of fresh experiments connected with the total quantity of the ingesta and egesta directed to the particular object, and of a refined chemical distribution of the elements taken into and passing out of the body. To this extent I conceive his labours to have been of great value; and it is well that Lavoisier's views should have received their confirmation and amplification at so able hands. I

* Lancet, vol. ii. No. 21. p. 725.

regret that it is necessary to dissent from not a few of the rest of Professor Liebig's views; and that I believe they will be found not only opposed to fact, but in many cases to each other.

III. Note to Pages 77. 82.

That the vital powers can so regulate the formation of the waste products discharged by the lungs, as materially to modify the quantity of heat which a given amount of carbon and hydrogen shall give forth in passing into carbonic acid and water, we must, I think, admit, upon the abundant proof which has come before our view. In tropical countries especially, there are states of the atmosphere, and habits of feeding, which require our admission of the fact, that although more than a certain quantity of heat cannot be yielded by a given quantity of carbon and hydrogen, they may, and they must often, form a union so controlled, and in such a manner, as to evolve much less than the utmost quantity of heat derivable from them. Of the causes of the development of heat in chemical changes, we as yet know far too little to lay down any absolute laws respecting them. Apart from the above compulsory reasons, there are many which demand the admission of the fact, that the heat developed in the production of a chemical compound appears, in a great measure, to depend upon the manner in which the union takes place. It is to be regretted that Professor Liebig maintains every where that the same amount of heat must always be developed by a given quantity of carbon, in what way soever its union with oxygen may take place. He even maintains (pages 89. 91. 94. 244. &c.*) that when any compound of carbon, hydrogen, and oxygen is transformed into carbonic acid and water, the heat evolved, however much of the required oxygen may be already in the compound, must be as great as if the oxygen were entirely derived from without; and he instances the very doubtful case of fermentation in proof of this position. What the amount of heat evolved in fermentation amounts to we do not know. It is very doubtful if it approaches to that of an equal transformation effected by combustion; nor do we know how far the relative capacities for heat of the saccharine basis, and of the alkoholic resultant, may have been concerned in evolving a portion of whatever heat is developed.

We have much better cases to appeal to in the destructive decomposition of vegetable acids by heat. If oxalic or citric acid, for example, are treated by heat, we know that they must be urged by a constant introduction of heat from without, in order to get them to burn: the heat yielded by the combustion is not sufficient even to keep up the action; whereas if a quantity of bituminous matter, containing an equal amount of carbon and hydrogen, be well triturated with a portion of nitre containing as much oxygen as would bring the mixture to the proportional composition of the acid, so that the carbon, hydrogen, and oxygen of this mixture shall be the same in quantity as in the acid, we shall find

* Organic Chemistry of Physiology and Pathology.

such a mixture, on a little application of heat, to burn spontaneously, in the manner named deflagration from its violence. It will not be said that the greater resistance of the acid to decomposition was the cause of this; for if it were greater, it would not affect the question of the little evolution of heat from the acid: but the affinity between the elements of the acid is in truth so feeble, as to require that it should be kept very dry to prevent its suffering spontaneous decay; while the bituminous compound may have been one of so strong internal affinity, as to undergo no spontaneous change in any length of time.

Many other cases might be instanced to prove that carbonic acid and water are formed from compounds containing their elements, without any evidence of of heat being evolved equal to that in the ordinary combustion of these elements, either singly or when united in the form of hydro-carbonaceous substances. When changes are effected by the power of galvanism, we have still less information as to the quantity of heat evolved or absorbed. In short, the question is one to be treated with great caution. We can only judge of the quantity of heat developed in the resolution of the elements of our food into carbonic acid and water, *by the result*, in the cases which come before our view. Now we have seen quite enough to render the habits of diet of both men and animals inexplicable, if we assume that the heat evolved from it is always equal to that produced by the active and unrestrained combustion of similar elements at a high temperature.

IV. Note to Pages 83, 84.

The universal fact, that the inhabitants of tropical climates are instinctively led to prefer a vegetable to a flesh diet, while the reverse is equally general in colder climates ; and furthermore that, in any climate, in proportion as the weather is hot or cold does the desire for a farinaceous or flesh diet prevail—this fact might, as already stated, caution us against pronouncing the farinaceous to be as much more productive of animal heat than the flesh diet, as it is richer in carbon and hydrogen than the latter. It will be difficult for any physiological authority to persuade common-sense observers that a rice, or even bread diet, is very heating, and one of beef as cooling ; yet such a view is every where set forth in the author's work—so rigidly does he adhere to the assumption that all the resolutions in the animal system, which terminate in the production of carbonic acid and water, must develope a quantity of heat proportional to the quantity of carbonic acid and water, and equal to that of high combustion.

V. Note to Pages 92. 95.

Considerable space is occupied by Liebig in the endeavour to establish a distinction between carnivorous and herbivorous animals, which, if it existed, would, as a necessary consequence, produce a total change in the habitudes of many animal races. It is maintained by him, that the whole of the flesh and blood consumed by carnivorous animals passes into living flesh and blood prior to its discharge from the body; but that, in herbivorous animals, that portion only of the food goes to the nourishment of the animal which is of the same elementary composition with its fabric, all the rest being either burned, or deposited as fat to be absorbed and burned,—that is, resolved into carbonic acid and water, with the production of heat. The argument is maintained in numerous passages, of which the following are some.

Speaking of carnivorous animals, he says (p. 60.), "The flesh and blood consumed as food yield their carbon for the support of the respiratory process; while its nitrogen appears as uric acid, ammonia, or urea. But, previously to these final changes, the dead flesh and blood become living flesh and blood; and it is, strictly speaking, the carbon of the compounds formed in the metamorphosis of living tissues that serves for the production of animal heat." Again: "The food of the carnivora is converted into blood, which is destined for the reproduction of organised tissues." And (p. 66.) he even proceeds as far as to say, "It cannot be disputed that in an adult carnivorous animal, which neither gains nor loses weight perceptibly from day to day, its nourishment, the waste of its organised tissue, and its consumption of oxygen, stand to each other in a well-defined and fixed relation."

With respect to the herbivora, he argues that the large quantity of non-nitrogenous principles in their food does not conduce to their nourishment, but is discharged in union with oxygen, producing heat in proportion, while the nitrogenous principles alone afford them nutriment; as in the following and many other passages. Speaking of the non-nitrogenous principles of their food, he says, "In these different substances there is added to the nitrogenised constituents of this food, from which their blood is formed, strictly speaking, only a certain excess of carbon, which the animal organism cannot possibly employ to produce fibrin or albumen;"—that is, cannot nourish the body with them. He then proceeds to instance the horse, to prove that all the non-nitrogenous matter it consumes is discharged from the lungs without having entered into the fabric of his body at all; and having shown how little proportional nitrogenous principles there are in the food of the graminivora, he observes (p. 75.), "It is obvious that in the system of the graminivora, whose food contains so small a proportion, relatively, of the constituents of the blood, the process of metamorphosis in existing tissues, and consequently their restoration or reproduction, must go on far less rapidly than in the carnivora. Were this not the case, a vegetation a thousand times more luxuriant than the natural one would not suffice for their nourishment."

These views, the reader will perceive, are directly opposed to those I have maintained in the text. Little, I hope, need be added to what is there said to refute them.

In the first place, we may ask, what evidence have we that the carnivora convert all their food into flesh, and that they form flesh at so prodigiously greater a rate than the herbivora, as the relative proportion of the nitrogenous principles consumed would indicate? There is no evidence whatever of this; on the contrary, the latter animals show a greater readiness to gain not only fat, but flesh also. And it is curious that the author should himself remark (p. 81.), speaking of the cow and sheep, — "Their system possesses the power of converting into organised tissues all the food they devour, beyond the quantity required for merely supplying the waste of their bodies. All the excess of blood produced is converted into cellular and muscular tissue : the graminivorous animal becomes fleshy and plump, while the flesh of the carnivorous animal is always tough and sinewy." It is, indeed, strange that this passage should follow so soon upon the former, where he says that a thousand fold vegetation would not suffice for even an equal production of flesh on the part of the herbivora. I must take leave here to remark, that these instances, in which the author's statements disagree with each other, either thus plainly in terms, or in their conclusions, are so numerous as to render it difficult to state what are his views on many points.

With reference, then, to the question before us, we must surely perceive the high improbability that there exists any *fixed* relation between the change of the animal fabric, the food, and the oxygen consumed. According to the author's argument, flesh diet ought to be the coolest nutriment, and, as I have already stated, selected, and not eschewed, by the native of the tropics; for with it he might gain his necessary nutriment, without any production of heat from other principles introduced with it; and a diet of rice and butter — that of the Hindoo — ought to be selected, as, next to oil alone, the one best suited for the Laplander, who would then not be burdened with excessive changings of his flesh, but would have an abundance of warmth from such diet. Next to him the Englishman, in winter, should have an instinctive desire for rice, and should shiver at the idea of tasting beef, which would not serve to warm him, unless he took great exercise to wear down enough of his animal fabric for supplying heat-producing elements. Liebig even takes this view, and attributes the constant motion of wild beasts in a cage, and the activity of savage huntsmen, to the need, on the part of carnivorous animals, of wasting their fabric for the sake of producing elements to warm them! In page 77. he remarks, "Man, when confined to animal food, respires, like the carnivora, at the expense of the matters produced by the metamorphosis of organised tissues; and, just as the lion, tiger, or hyæna, in the cages of a menagerie, are compelled to accelerate the waste of their organised tissues by incessant motion, in order to furnish the matter necessary for respiration, so the savage, for the very same object, is forced to make the most laborious exertion, and go through a vast amount of muscular exercise." We must not allow ourselves to be misled by any such specious facts. The incessant motion of the lion, tiger, &c. arises solely from their impatient ferocity. When quite tamed they become very sluggish. In their wild state, they lie still much more than herbivorous animals, which are almost all day on their legs; while the beasts of prey, as the Psalmist long since rightly observed, "gather themselves together, and lay them down in their dens."

The most striking instance of incessant motion is that of *a purely herbivorous* animal, the elephant; and, with respect to human labour, the total amount of muscular exertion of the husbandman very far surpasses that of any savage. The exertion of hunting is occasional only. The unwillingness of savages to go through the toil of husbandry is a great obstacle to their civilization. Their habits are those of general indolence, with intervals of exertion. With respect to the opinion above quoted, that *all flesh diet* is converted into living fabric before it can pass off by the lungs as fuel,—let us reflect upon the case of a man going through little exertion, and living in a cold climate, as that of many in England: can we imagine that if he were fed solely on flesh, his vital powers would be compelled in cold weather rapidly to convert the whole of his food into flesh—going through the elaborate and laborious processes of forming myriads of fibres, of vessels, and of nerves, more than would otherwise be needed, only that they may be taken to pieces again to supply elements for producing heat!

Or again, in the case of a Laplander or Greenlander,—are we to be told that when he feeds on blubber and other non-nitrogenous, or nearly non-nitrogenous principles, he maintains his animal heat by employing a large part of this food directly as fuel; but that when he lives chiefly on *flesh*, and not fat, he is compelled, merely because a flesh diet happens to correspond with his own flesh in composition, to convert the whole of such diet into living fabrics before it can be employed to generate heat? Are we to imagine that in those borean latitudes the body runs through its changes so rapidly, when the diet happens to be flesh, as to have some pounds weight of living vessels, nerves, fibres, &c. built up and pulled down again daily, merely to supply the animal fuel requisite? The idea is not to be entertained for a moment. As already stated in the text, the simple and reasonable conclusion is, that of the food taken the quantity converted into living fabric is not determined by the proportion of the food which happens to correspond, in elementary composition, with that fabric. A man is not obliged to form a pound of living muscle, &c. because he eats a pound of muscle; but the quantity of the food vitalized (if we may use the term) is determined by the wants of the system. If he needs little change of his bodily fabric, as when sedentary in England, or half torpid in a Lapland hut, the vital powers will trouble themselves only with a correspondingly small conversion of food into living matter; the greater part of it, whether it contains nitrogen or not, will be directly employed in the maintenance of his animal temperature, without passing through the circuit of forming living matter.

Next, with respect to the author's opinions upon the changes which the food of herbivorous animals undergoes. In the passage above quoted, and in numerous others, he contends that only the nitrogenous principles are convertible into their animal fabric; and that all the rest is employed directly as fuel, having first wholly, or in part, gone through the form of bile. In the text I hope it has been abundantly proved that any so exclusive a doctrine cannot be admitted, facts on all hands being opposed to it. We must view their case as differing only from carnivorous animals, in their having to provide themselves with nitrogen for converting into their fabrics products wanting this element, whensoever their food has not enough of nitrogenous principles to nourish them; and that, as in the case of the carnivora, the quantity of the food employed directly as fuel is not perniciously determined by its containing or not containing nitrogen, but is regulated by the wants of the animal at the time.

VI. Note to Page 117.

Unless we allow to the vital powers the ability to draw upon the free nitrogen which enters the blood in respiration, and circulates freely throughout the system, or unless we imagine that some of the nitrogen of the absorbed fabrics can be employed again (which is less probable), we are driven, as already stated, to hold the opinion that only those principles in food which contain nitrogen can serve for the nutriment of the body, and we can only account for the expenditure of all the non-nitrogenous principles as fuel. This view, we have seen, is quite untenable; it is opposed to the instinctive habits of diet of nearly all regions. It renders the case of animals, such as the horse, quite unintelligible. Indeed, it proposes to our adoption propositions so incredible, and so opposed to all facts, that it cannot be received. We have seen that the horse and the Hindoo must draw nourishment from non-nitrogenous food; and I am sorry to find Professor Liebig so often to maintain a contrary opinion, as in passages already quoted, and

p. 95. where he says, "the substances of which the food of man is composed, may be divided into two classes; into *nitrogenised*, and *non-nitrogenised*. The former are capable of conversion into blood, the latter are incapable of this transformation." This view pervades a section of his work, and is applied to the food of other animals. Yet the opinion is supported by no other proof than the *presumption* that the vital powers cannot turn to account nitrogen found in the system, and cannot modify the principles of the food at all, merely because some of the food happens to be of a nature similar to the bodily fabric, and therefore already assimilated. That the vital forces have not the power to assimilate any principles of the food, but can only employ for nourishment such as are already assimilated to the fabrics, is an assumption in the highest degree improbable in itself, and opposed by facts on all hands, as we have abundantly seen. Nor can we allow the least force to any arguments rested on the nature and quantity of the gases respired by the brute creation. When we consider how very difficult it is to arrive at any near estimate upon these points in the case of human beings, who aid our experiments with their intelligence, what value can be attached to *the figures* which some physiologists have been pleased to publish about the respiration of unwieldy, unmanageable brutes? It is impossible not to regret that Liebig should ever found arguments upon them.

VII. Note to Pages 87. 90. 92. 94. 148.

It is here necessary to remark upon a singular doctrine propounded by Liebig, which may be stated in his own words. "The change of matter * (*i. e.* of the animal tissues), the manifestation of mechanical force, and the absorption of oxygen, are in the animal body so closely connected with each other that we may consider the amount of motion, and the quantity of living tissue transformed, as proportional to the quantity of oxygen inspired and consumed in a given time by the animal. For a certain amount of motion, for a certain proportion of mechanical force consumed as vital force, an equivalent of chemical force is manifested; that is, an equivalent of oxygen enters into combination with the substance of the organ which has lost the vital force, and a corresponding proportion of the substance of the organ is separated from the living tissue in the shape of an oxidised compound." Again (page 237.): "The production of force for mechanical purposes, and the temperature of the body, must consequently bear a fixed relation to the amount of oxygen which can be absorbed in a given time by the animal body." This view will be found to pervade the latter part of the work, and to form, indeed, the basis of the matter of that part.

It appears to me that the physiologist, who will be at pains to subject this and many other of the author's speculations to cautious examination, must pronounce them ultra-chemical, and wholly untenable. The

* Organic Chemistry, p. 222.

present doctrine, and all the conclusions deduced from it, have, and can have, no other foundation than the familiar matter of observation, that a loss of flesh and a quickened respiration are attendant upon exercise and labour. The author, we find, assumes that each one of these bears so close a relation to every other of them, as to be the measure of it; and he employs the singular view he entertains of the ever-vigilant efforts of oxygen to consume the body, to explain the manner in which muscular efforts give rise to a waste of the fabric. Page 222, he says, "How, indeed, could we conceive that a living part should lose the condition of life, should become incapable of resisting the action of the oxygen conveyed to it by the arterial blood, and should be deprived of the power to overcome chemical resistance, unless the momentum of the vital force which had given to it all these properties had been expended for other purposes?" and again p. 223, "As long as the vital force of these parts is not conducted away, and applied to other purposes, the oxygen of the arterial blood has not the slightest effect on the substance of the organized parts; and in all cases, only so much oxygen is taken up as corresponds to the conducting power, and, consequently, to the mechanical effects produced." "The oxygen of the atmosphere is the proper, active, external cause of the waste of matter in the animal body; it acts like a force which disturbs, and tends to destroy, the manifestation of the vital force at every moment." This view prevails wherever the author has occasion to refer to the action of oxygen. Though there are passages which admit its subserviency to the vital power, and which, therefore, so far disagree with the rest, and although the author of course treats of oxygen as a necessary agent for the production of heat, and for these operations in which he conceives it to act in concert with the wants of the system, his prevailing argument is that the action of the vital force, in respect to the oxygen circulating in the system, is not to enforce and direct its action, but to control and check it; and that if the vital force be at all enfeebled or withdrawn, the oxygen will attack the bodily fabric. He then contends that exercise and labour consume vital force, and thus leave the muscular and other fabrics a prey to the oxygen.

I cannot imagine that the able author himself will long entertain such views. In the first place, we have had plentiful evidence, in the preceding pages, that heat is largely produced from materials derived not from the bodily fabric, but directly from the food, with a proportional consumption of pulmonary oxygen. And the author, in many parts of his work, has maintained this unquestionable fact at considerable length ; though in other places, as in the present, he confines the consumption of oxygen and the production of heat to the proportion of waste fabrics removed. The one view is incompatible with the other. There can be no doubt that in numerous instances heat is largely produced, both in men and animals, as well while active as when resting, from materials supplied direct by the food, and that the production of heat is by no means confined to a waste of the fabrics. The author has himself shown that in the horse a large part of the food passes off by the lungs, without ever forming flesh or other living animal matter. He has carried this view much further than I have found it possible to follow him; for he has contended that all the non-nitrogenous portion of the food, forming the great bulk of it, has this destination; and has even argued that the change of the animal's fabric must be very small — much smaller than in man. But his present hypothesis requires that no oxygen whatever should be consumed, nor any heat at all produced, but such as is connected with a wear and tear of the fabrics; otherwise the hypothesis is at once vitiated.

Again, in order to find a reason for the supposed rigid connexion between the heat developed, the waste of the fabric, and the exertion of force, as in labour or exercise, the author has recourse, as above noticed, to his peculiar view respecting the predatory power of oxygen.

Can we allow ourselves to imagine that in nature any so clumsy arrangement should exist,—with reverence be it said,—as that of introducing an agent into the system which seeks to consume the living fabrics, not in immediate obedience to the vital power, but the instant its vigilant protection is diverted for a moment; and not for the good of the animal system, but at its expense, and solely to satiate its own powerful affinities? It is surely almost unnecessary to controvert such an hypothesis. We must not allow ourselves to suppose that any action takes place but what is in obedience to (not in the absence of) the vital powers. We often perceive, at a period when a disease takes a turn, a person is in the lowest state of debility, with his vital powers almost exhausted; — at such a period, the hypothesis which gives to oxygen such predatory power over the body would leave one so enfeebled without a chance of escape; yet we find such an one to *gain* flesh often rapidly. Without a doubt there is no dissolution of the fabrics but what the good of the system requires, and what is *directed* by the vital powers. We must hold the action of oxygen in the blood to be altogether subservient to, and directed by, the vital powers ; hence, although in the larger arteries there are present, with oxygenous particles, an abundance of others for it to prey on, and though these are not living matter, nor guarded by the vital power, excepting where they may touch nervous points on the inner surface of the vessels, yet little action goes on. It is not until the fluid passes into capillary tubes, and comes fully under control of the vital power, that a mutual action takes place between the oxygenous and other particles. So entirely is the action of oxygen dependant upon the vital power, that in the larger vessels, where the latter operates feebly, it does not proceed.

But even in the case of labour, the fact which at first sight gives countenance to, and is, no doubt, the foundation of Liebig's hypothesis,—namely, that development of heat, waste of the fabrics, and consumption of muscular force, appear to proceed together ; this apparent fact will not, by any means, bear close examination. Nothing is more common than to find a person at the commencement of a course of labour to be greatly heated, — to respire deeply, taking into the system much oxygen, and to lose flesh so quickly under it as to grow thin. So far his case appears to tally with the hypothesis ; but in time, when the person has become accustomed to it, the same, or even greater labour, will be regularly performed by him without any such effects, and flesh will even be regained under it: also the appetite will often grow more moderate. Young men undertaking a laborious tour on foot are a familiar example of this. The development of heat is increased; the appetite is often greatly excited, where the labour is not excessive; and loss of flesh also takes place, and at first rapidly: in time the daily labour is easily performed, — both the wasting ceases, and the appetite becomes more reasonable. In animals the same thing may be observed. The horse recovers flesh under labour which wasted him at first, and performs it without the greatly increased production of heat.

These familiar facts are enough to clear away the notion at once, that there is any necessary and proportional connexion between the amount of force exerted, the waste of the fabric, the production of heat, and consumption of oxygen. That there is a connexion is true, but it is one of coincidence merely. In proportion as the action of all the voluntary muscles accelerates the circulation, it multiplies the number of particles travelling the capillaries in a given time. Thus much more action between particles must take place, and the supply of them is kept up at the same time by the return into the blood of an increased quantity of matter; for the increased flow of blood stimulates the nerves and minute vessels, and amongst them the absorbents, to greater activity. This familiar view affords us a ready and sufficient explanation of the production of heat, and the loss of flesh attending labour, so long as the exertion has the effect of materially accelerating the circulation. As soon, however, as the heart ceases to be excited, and the smaller vessels also bear the pressure of the fluid without increasing their action, the quantity of action throughout all parts of the system diminishes: there is then less laboured respiration, less development of heat, and a less change of the fabric of the body. Thus we perceive that labour may or may not be attended with a corresponding production of heat, consumption of oxygen, and waste of the fabrics, according as the person is or is not accustomed to it. The relation, then, that it bears to all these actions is an incidental, and not a dependant one. This view is reasonable and intelligible, and far more in accordance with the observed proceedings of nature, than the notion that there is only vital power enough in the system to protect the muscles from the ferocity of oxygen which had been (unaccountably) allowed to enter the system, and take up its position every where; and furthermore, that the stock of vitality being thus limited, directly any is diverted by the will from its custodiary duty to produce motion, some of the animal fabric falls a prey to the oxygen. Were such the case, we might indeed wonder why so much oxygen had been allowed an entrance,—why the portals had been thrown open to it until it occupied, and tyrannised in, every alley of the city.

As a consequence of this extraordinary view, it would follow that the quantity of heat generated, and of oxygen consumed, would be a measure of the muscular action of an animal, and would determine the comparative force exerted by two different animals. Liebig has discerned this fact, and has instituted a comparison between the horse and the whale in these words: — "The quantities of oxygen which a whale and a carrier's horse can inspire in a given time are very unequal. The temperature, as well as the oxygen, are much greater in the horse." *

We might have expected that this truth would have served to show that the hypothesis of the muscular force corresponding with the heat and oxygen was altogether incorrect, for it would make the horse possessed of more absolute power than the whale; but so strongly is the author attached to the hypothesis, that he even concludes upon its strength that the horse actually is the more powerful animal; as follows: — "The force exerted by a whale, when struck with a harpoon, his body being supported by the surrounding medium, and the force exerted by a carrier's horse, which carries its own weight and a heavy burden for eight or ten hours, must both bear the same ratio to the oxygen consumed. If we take into consideration the time during which the force is manifested, it is obvious that the amount of force developed by the horse is far greater than in the case of the whale." †

The argument is here not clearly stated. It commences with the fact, that in equal times a horse consumes more oxygen than a whale, and consequently, in equal times, the former must develope more force than the whale. To confine the period of the comparison in the one to that of the effort on being struck by the harpoon, and in the other to labour all day, confuses and spoils the argument.

* Organic Chemistry, p. 237. † Idem.

The conclusion of the argument which the hypothesis plainly requires is, that the horse must be considered as the more powerful animal, not relatively only, but absolutely. Now, it is almost unnecessary to show that this is utterly contrary to the fact. In the first place, the heavy medium, though it supports the weight of the whale, offers to its movements a prodigious resistance, especially to any quick motion. A broad ford, or flooded road, though the water offers resistance to the small surface of a horse's legs only, is found to weary him greatly; and if the water is above his knees, he becomes quite exhausted in wading a couple of miles, or less. Compare with this the huge surface of a whale deeply immersed in the sea, and propelled by the force of the animal at great speed during a whole day.

A whale may often be discerned spouting near the horizon, far astern of a ship, in the morning, and in the course of a few hours it will overtake a quickly sailing vessel, and be out of sight ahead of it. The force of *a hundred-horse-power* engine would not suffice to propel it at such speed. Yet it will pursue its course for a length of time without resting. In short, the amount of respiration and of animal heat, and the development of force, bear no proportion whatever to each other in the two animals; so that, directly a horse and a whale are named together in such a view, we ought to perceive the hypothesis, which would make the force and the oxygen consumed a measure of each other, to be unsound. This conclusion is no other than we might expect, from the prior evidence we have had in the case of persons growing accustomed to labour which caused much production of heat at first. It is also in accordance with every reasonable expectation that exertion of force, and warmth, should have no such absolute relation as to be inseparable, — as to require for the maintenance of the temperature necessary for life that the vital power of the system should be wasted, and some of its curious and highly-wrought fabric pulled to pieces.

VIII. Note to Pages 73. 96. 105.

In several parts of Professor Liebig's work we find him to maintain a doctrine which I cannot but consider as altogether fanciful, and unsupported by fact. It is, that the vital force of plants exceeds greatly that of animals, which he attributes to so much of the force of the latter being called away and expended on involuntary and voluntary motion. It is true that animals do not nourish themselves upon carbonic acid, because it pleased the Creator to provide another arrangement for their nutrition; but there is abundant proof that the strongest chemical affinities yield to the vital force of animals. There cannot be a question that constantly fat is formed, when there is plenty of oxygen floating in the blood, the affinity of which, both for carbon and hydrogen, is opposed to such formation of fat, but is overruled by the vital force. So also is the very compound fabrics of the body preserved by the vital force, although numerous chemical affinities are at work to alter them.

Nearly all vegetable principles and juices, — gums, resins, oils, and even extracts, — are of a nature, when removed from the protection of the plant's vitality, less liable to decay than animal principles, their constitution being less at variance with chemical affinities; which shows that they owe less to the protective power of the vitality of their parent plant, than do animal secretions to the protective power of the animal vitality.

As to the consumption of vital force in muscular action diminishing the animal vitality, it is a doctrine purely hypothetical, and by no means supported by facts. We do not find the most sedentary animals to have the organization of their fabric more perfect, and more able to resist outward causes of decay, than the fabric of animals constantly in exercise; but the very contrary. More might be said on this subject, but I conceive it to be unnecessary.

IX. Note to Page 7.

When the stomach is distended with air, the oppression and suffocative feeling produced must be considered as chiefly mechanical. The diaphragm being pressed up towards the chest, the lungs are so compressed as to lose much of their gaseous capacity and vascular freedom.

The obstruction of the circulation and oxydation of the blood has its usual effect on the brain and heart, which is serious or trifling according to the oppression. When the air distending the stomach is of a poisonous nature, it will, of course, act through the nervous system according to its virulence. Upon the subject of stomachic distension, the work of Professor Liebig contains extraordinary views. In p. 115. we find him to say, "If we consider the fatal accidents which so frequently happen in wine countries, from the drinking of what is called *feather-white wine*, we can no longer doubt that gases of every kind, whether soluble or insoluble in water, possess the property of permeating animal tissues, as water penetrates unsized paper. This poisonous wine is wine still in a state of fermentation, which is increased by the heat of the stomach. The carbonic acid gas which is disengaged penetrates through the parietes of the stomach, through the diaphragm, and through all the intervening membranes, into the air-cells of the lungs, out of which it displaces the atmospherical air." Again (p. 116.): "No doubt a part of these gases may enter the venous circulation through the absorbent and lymphatic vessels, and thus reach the lungs, where they are exhaled; but the presence of membranes offers not the slightest obstacle to their passing directly into the cavity of the chest."

Fully as it behoves the physiologist to bear in mind the property of exosmosis, and to give its operation a due place in his science, few, I conceive, can fail of being startled by such an account of it. If we carry our minds round the whole circle of cases in which a transudation of gaseous fluids can be supposed to occur, there are assuredly none which give countenance to so extravagant an opinion as that here maintained. There is all possible evidence that any one single mucous, and especially any serous membrane of the denser kind, does in life offer a remarkably powerful obstruction to the exudation of gaseous fluids; and it is well that it is so. Thus gaseous fluids of the most offensive odour will exist at all times in the intestinal cavity of many persons, without any finding their way to the surface, or to the lungs, so far even as to be perceptible to the olfactory sense. Yet a quantity will give fetor to the breath, far smaller than can easily be conveyed to the lungs by absorption into the circulation. The mere passage of the breath over offensive secretions of the fauces and mouth, especially of the tongue, is often enough to render breath fetid, which was not so in the trachea; for cleansing the surfaces well will, in such cases, frequently remove the odour. We have no instances of their passing even into the cavity of the peritoneum, so as to affect the character or odour of the serous fluid there. In the case of colic and tympanitis, we have great and oppressive distension, continuing for many hours, and sometimes many days, which could not exist if the membranes and tissues offered "not the slightest resistance" to the passage of gaseous fluids; if they did not, indeed, offer a resistance so great as to require the removal of the fluids by the vital process of absorption.

Can there be a doubt that they are removed by absorption and not by exudation, when we bear in mind that, if gaseous fluids passed out by the latter process, they would have to pass into, and through, at least one cavity, that of the abdomen?

Can we for a moment admit the notion that the permeability of each one of the tissues is so exactly equal to that of every other, that no partial detention of the gas would take place anywhere, so as to inflate the obstructing membrane? Thus, when the gaseous fluid had passed the mucous and muscular coats, would the fine peritoneal coat not so far check its progress as to cause it to swell up the cellular tissue under that coat?

Again, when the gas had reached the abdominal cavity, are we to be told that it would pass through the peritoneal lining of the abdomen without at all inflating the cavity, so easily as it is inflated? The smallest reflection forbids our entertaining any such notion. Yet we do not find the abdominal cavity even temporarily occupied by gas, when a colic or tympany are giving way. Before the gas too escaped by the skin, do we not know well that it would, as in emphysema, blow up the cellular tissue, especially when we consider how large often is the quantity collected in the intestinal canal?

The case imagined by the author, that the gas passes into the lungs, is less credible still; for, in addition to all the obstructions briefly alluded to above, we have the dense pleura over the diaphragm, which being passed (if that could take place) the gas would be received into the cavity of the pleura, where we know any gas entering causes a proportional collapse of the lung, and remains for a time lodged between the pleuræ, until *gradually absorbed.* Lastly, admitting, for the sake of argument, an occurrence opposed to all experience, that the gas could pass through into the air-cells and tubes, are we to believe that it could not only *ooze*, but actually be *blown* through all the membranes, tissues, and muscular layers, with so great rapidity as to overtake the instinctive effort, which its suffocative action would produce, to clear the chest of it in two or three hurried breathings? How are we to imagine the patient so indifferent as to allow his lungs to become suffocated by the oozing in of a gas, which is the very same as that exhaled from the blood in large quantity, for the removal of which he is provided with ample power by the process of deep respiration, and with a ready sensibility of its presence in the feeling of suffocation, — in the feeling which the very same gas excites when we instinctively yawn to clear our chest of its excess? A few yawnings would overtake all the fermenting power of the feather-white wine, supposing the gas could enter the lungs.

Again, if so numerous layers of strong and dense membranes offer so little resistance to the transudation of gases, why should the membrane lining the finer air-tubes and cells of the lungs be given the extreme delicacy we know it to possess? Exposed as it is to irritating and acrimonious particles gradually introduced in respiration, it would undoubtedly have been much less liable to irritation and disease had it been a tougher and coarser membrane. We know that in that case, far from offering "not the slightest resistance," it would have fatally obstructed the absorption of atmospheric air, and the exudation of carbonic acid.

What proof, then, we may well inquire, has the author of the strange assumption, that persons, poisoned by fermenting wine, die of suffocation, from the rapid passage of carbonic acid gas direct from the stomach to the lungs, through about twelve membranes and tissues? The following is the proof he gives: — "The surest proof of the presence of the carbonic acid in the lungs is the fact, that the inhalation of ammonia (which combines with it) is recognised as the best antidote against this kind of poisoning."*

From the pen of any author of less merit, such a passage as this it would be well to leave without comment; but the deference which Liebig's valuable researches and discoveries in organic chemistry command, gives weight to every thing passing, however hastily, from his hand. Had he been led to consider well the statics of the chest, he must have perceived the fact, that when ammonia is inhaled no appreciable quantity of the gas reaches the air-cells. It could not do so, without many more deep, and repeated, inhalations being practised, than could be tolerated. In fact, were any quantity of this acrid gas to reach the cells, it would be no less suffocative than carbonic acid, as displacing oxygen; while it would be more poisonous and irritating. Even if it did reach the cells and combine with that gas, would gaseous carbonate of ammonia in the air-cells be in any ways preferable to gaseous carbonic acid? Rather would it not be far more suffocative and destructively irritating? Lastly, is it not obvious, if all the remedy requisite is the saturation of the carbonic acid with ammonia, that the proper place in which to effect this would be the stomach, where the gas was being generated, and that that organ could receive, every few minutes if necessary, by way of the mouth, a hundred fold larger dose of ammonia, than could by

* Organic Chemistry, p. 116.

any possibility be carried down into the air-cells by inhalation, if, indeed, any at all could be conveyed into them? The action of inhaled ammonia in such cases, whatever it amounts to, is upon the Schneiderian membrane, fauces, larynx, and possibly also the trachea; and it must be considered as altogether that of a stimulant, acting upon and rousing the circulation through the nervous system and the brain.

Such a simple consideration of the question as has been pursued above, makes it evident that any other view is wholly inadmissible.

In the preceding examination of Professor Liebig's views, the reader will have observed that it has been extended only to those which relate immediately to questions discussed in the present work, and which, therefore, proceeding from so recent and high an authority, could not be left unnoticed. With a profound respect for the author's powers of chemical research, it is not without much regret I have found it necessary to differ from him so widely, and on so many points. Upon these points every physiologist of any learning, and power of reflection, may form a judgment for himself; and to each such I would leave the issue, with the recommendation that he would afford the subject the attention requisite, and not rest upon any authority, however deservedly high in other respects, for the opinions he forms.

There remains one point which I feel unwilling to leave unnoticed, though it is not immediately connected with my subjects. I refer to Liebig's views respecting the use and action of alkohol in the system. I cannot refrain from protesting against them, as here also ultra-chemical. In page 239. he observes, "There can be no doubt that the elements of alkohol combine with oxygen in the body; that its carbon and hydrogen are given off as carbonic acid and water." And again: "It is consequently obvious that by the use of alkohol a limit must rapidly be put to the change of matter in certain parts of the body. The oxygen of the arterial blood, which, in the absence of alkohol, would have combined with the matter of the tissues, or with that formed by the metamorphosis of these tissues, now combines with the elements of alkohol." In these, and other passages of a similar kind, we find him, in the first place, to leave unnoticed the great and obvious effect of alkohol upon the nervous system; secondly, to dwell solely on the supposed use of its elements as animal fuel; and thirdly, to rest the proof on the assumption that alkohol undergoes an entire decomposition in the system. With respect to the first, it would be trifling with the observation of any reader to enter into proof that the main action of alkohol is not that of the mere carbon and hydrogen it contains in common with a proportional quantity of oil, but is due to its proximate and compound properties as a powerful narcotic stimulant acting directly on the nervous system, compared with which action any ultimate use of its decompounded elements is as nothing. Secondly, with respect to the author's estimate of the power and use of alkohol solely by the animal fuel, the carbon and hydrogen, which it contains,—it must be to every reader of the author's work beyond measure surprising that in this, and in other instances, various powerful agents are measured chiefly by the value of their elements as materials for yielding carbon and water, or intermediately forming some animal secretion, as bile. We have a remarkable instance of this in page 181.; where, after proving, or rather stating, *theine*, the essential basis of tea, and *caffeine*, the basis of coffee, to be one and the same principle, he shows that by the addition of oxygen and of the elements of water they may pass into bile; or rather, that the arithmetical sum of the elements of either, and of an arbitrary quantity of oxygen and of water, may be made to correspond with the sum of the elements in bile. Since nearly all varieties of animal and vegetable principles derive their materials from four elements only, if we are permitted to shift two out of the four quantities *ad libitum*, it does not appear to me very surprising that we should be able to make two compounds agree, each of which contains the whole four elements. I cannot, therefore, find reason for attaching the smallest weight to the fact (without questioning its truth) that the sum of the elements of theine, and of an assumed quantity of oxygen and of water, equals the sum of the elements of bile. This, however, is not the remarkable point at issue. It is, the importance Liebig attaches to the fact that *theine* and *caffeine* may form bile in the system. Not satisfied, though he refers to it, with the peculiar influence of these grateful stimulants (or, as we are told, this stimulant) on the stomach and nervous system, he dwells upon the probability of their chief use as affording materials for bile; and this, although, as he admits, their quantity would only be a small fraction of a grain, while the daily production of bile may exceed a pound. In fact, any one mouthful of meat a person eats supplies more of the elements of bile than all the *theine* in the tea he swallows in a week.

To return to the question of alkohol. Can we bring ourselves to view the carbon and hydrogen in half a pint of brandy as of any separate effect at all to be compared with the highly stimulating power of that quantity of spirit? Half a pound of bread would be as effective as animal fuel; nay, far more so, inasmuch as it would be easily decomposed, whereas alkohol is in a singular degree indigestible. This leads us to remark upon the third point, — the evidence on which he grounds his view of the use of alkohol.

We find him to contend for its complete digestion and resolution into carbonic acid and water, upon the assumption that it does not pass off undecomposed by any of the outlets of the body. If we begin with the first process of digestion, that in the stomach, we are assured, by the careful inquiries of Doctor Beaumont and others in America, that alkohol is not at all resolved by the gastric fluid, but is absorbed unchanged into the blood. This is an important fact, rendering the subsequent resolution of it very questionable. Again, it is found excreted from the blood, and deposited with the exhalations from membranous surfaces, as in the ventricles of the brain. It has been there found in so large proportion as to render the ventricular fluid inflammable, as in the case observed by Doctor Brown of Edinburgh. Beyond such facts as these, once established, I do not think it would be necessary to proceed. They abundantly disprove the opinion that alkohol is readily digested, and transformed in the system; and prove that, in the effort to get rid of it, nature even employs the ventricular membranes in the brain to exhale it. From this we might be satisfied, that although the peculiar sensibility of the kidneys and bladder disqualifies them for carrying off, or bearing, the stimulus of any but urinary irritants, of which there are many other instances, and although, therefore, we need not expect to find alkohol in the urine, we may be sure it will find vent by the skin, and the lungs. The shirt of a drunken soldier in the tropics, sweating profusely in a debauch on arrack, has a strong odour of the spirit, of which his skin is relieving him; and in proportion to the profuseness of his perspiration can he endure the ingurgitation of liquor, which is thus obviously carried off unchanged. So in our damper climate, a large portion of the spirit swallowed by the profligate inebriate fumes forth unchanged from the lungs, and in such quantity as to render every breath offensive.

Yet the author, in page 239., takes it for granted, upon supposed observation, that alkohol does not pass off by the lungs or skin! This is the more curious, since we find in the same page the following statement respecting the escaping power of alkohol, in which he goes even further than most will feel able to follow him:—"Owing to its volatility, and the ease with which its vapour permeates animal membranes and tissues, alkohol can spread thoughout the body in all directions." Surely, by the time we admit all this, we must view the spirit as passing not by excretion only, but even *sua sponte*, from membranes it so easily permeates,—the pulmonary and cutaneous; thus making its escape from the body as from a heated sponge, by volatilization. In this passage the author invites us, though not intentionally, yet of necessity, to conceive a discharge of alkohol unaltered to take place more rapidly than I should even desire, in order to disprove, his view.

It is right that no deceptive notions should gain force giving a false value to alkohol. However allowable may be a cautious use of it in the form of some weak fermented liquors, in the form of distilled spirit it should be altogether discountenanced, excepting as a narcotic stimulant, proper only in the hands of the physician.

NOTE TO THE DEDICATION.

This Essay was entitled "An Inquiry into the Comparative Forces of the Extensor and Flexor Muscles connected with the Joints of the Human Body." Its object was to controvert a prevalent opinion, established on the high authority of Richerand, that the flexor muscles were in all the larger joints more powerful than the extensors. As a question in science only, an erroneous view of the action of most of our joints is not unimportant; but this had an immediate reference to a part of surgical practice of the first moment, the treatment of fractured limbs, and diseased or injured joints. Not questioning that on the part of all superior men a perfectly correct treatment prevailed, it is obvious that so long as the *reason* assigned for the position given to an affected limb was unsound, the practice must have partaken, in that particular, of an empirical character.

The work was not publicly announced at the time, but chiefly circulated by friends. I am not aware that it was reviewed or at all generally known. Since the erroneous doctrine has been since taught in some quarters, I have thought of a republication of the arguments with other matter since collected, as "An Essay on the Local Treatment of certain Affections." Since, however, leisure may not be afforded me, I would commend the particular question above referred to, to the attention of any readers, who may have received the controverted opinions. I think they cannot fail, on due reflection, to arrive at the conclusions to which I was led; or it may be allowable for me to appeal at once to some of the letters then received by friends, which establish, upon the highest authority, the correctness of the view taken, and the arguments by which it was supported. I could not do so without at the same time expressing my consciousness of the terms employed having been altogether beyond my merits, and of their having flowed from the generous desire of great minds to encourage the early efforts of a youth. Mr. Travers (to a friend), "I have perused this little volume with much instruction and pleasure. I have no hesitation in saying that the Author demonstrates his statements very satisfactorily; and I can only express my regret at his leaving Europe both on professional and personal considerations. We have few young men equally competent to inform us." Mr. Abernethy (to a friend), "I think Mr. Jeffreys's work will do him credit with the Profession, for it shows a considerable extent of anatomical knowledge, and also that he is a thinking and reasoning character. Moreover, I believe that his readers will acknowledge the truth of his facts and arguments." Sir Astley Cooper (extract to a friend), "Pray make my best compliments and thanks to Mr. Jeffreys, and congratulate him upon the * * * * * promises of his son. The work evinces a great deal of thought and talent." Such expressions towards one personally unknown to the writers, though as undeserved as they were generous, do nevertheless show, upon the highest authority, that the subject is one of importance, and that, in their opinion, it was correctly handled.

THE END.

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